A
FULL CONFUTATION
OF
Dr. JURIN's Reply
TO THE
REMARKS
ON HIS
ESSAY
UPON
Diſtinct and Indiſtinct VISION.

By *BENJAMIN ROBINS*, F.R.S.

Quid faciet in geometria, qui non dedicerit? —— aut, taceat, oportebit, aut ne ſanus quidem judicetur.
Cic. de Orat. Lib. 3.

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PREFACE.

AS the gentleman, whoſe errors are the ſubject of the enſuing tract, has accuſed me in his Reply to the Remarks, I had publiſhed on his Eſſay upon diſtinct and indiſtinct Viſion, of conducting thoſe animadverſions with paſſion and abuſe, and as this charge, were it true, would be a much heavier imputation on me than any exceptions, that could be made to the ſcientific part of my writings; I muſt beg leave, while I defend my former judgment of this author's favourite production, to lay before the reader a ſuccinct account of our mutual behaviour in thoſe diſputes, that ſome time paſt interveened between us.

ABOUT ſix years ſince a pamphlet was publiſh'd under the title of the Analyſt; in which the author endeavours to ſhew, that the doctrine of fluxions invented by Sir Iſaac Newton is founded on fallacious ſuppoſitions.

AS that writer had a falſe idea of this doctrine, repugnant to the deſcription of it given us by its great inventor; I thought the moſt effectual method of obviating his objections would be to explain in a more diffuſe and circumſtantial manner, what Sir Iſaac Newton himſelf had delivered with his uſual brevity relating to the genuine principles of this ſcience. And with this view I publiſhed a Diſcourſe on Sir Iſaac Newton's methods of fluxions, and of prime and ultimate ratios.

BUT in the mean time a controverſy was carrying on between the author of the Analyſt and another, who under the name of Philalethes Cantabrigienſis had undertaken the defence of Sir Iſaac Newton: and as I at laſt perceived, both by the *conceſſions of Philalethes, and the avowed opinions of others, that the erroneous conceptions of the writer of the Analyſt on this head were more prevalent even amongſt thoſe, who approved of the method of fluxions, than I had at firſt believed; I thought, it might be no unacceptable task more particularly to ſhew thoſe, who were thus miſled, how irreconcileable their opinions were with the tenets of Sir Iſaac Newton, and how impoſſible it would be to defend the accuracy of his doctrine on theſe their miſtaken ſuppoſitions; and it was with this intention, that in an account of my book inſerted in the Preſent State of the Republick of Letters, ſome of the errors contained in the writings of Philalethes Cantabrigienſis were endeavoured to be obviated.*

BUT tho' this diſcourſe was written with great caution, and only mentioned the principles objected to without ſo much as naming or even inſinuating the treatiſes, from whence they were taken; yet, as Dr. Jurin, who was generally reputed the author of them, was one, that I often converſed with; at my requeſt, before this paper was printed, a common friend carried to him the manuſcript, and, without pretending to ſuppoſe, whether he was, or was not Philalethes, deſired him to read it, and asked him, if he thought, Philalethes could be diſpleaſed with any thing contained in it; he was alſo told at the ſame time, that if he believed, any part of it could give offence to that gentleman, whoever he were, it ſhould be ſtruck out, or that I would even let the whole deſign fall, if he deſired it.

MY friend brought me the Doctor's anſwer importing, that he could not believe, my paper would

would diſpleaſe any one; ſince, if the tenets, I excepted to, were really erroneous, it was reaſonable, they ſhould be expoſed; and if otherwiſe, it was the buſineſs of Philalethes to defend them. However in any caſe Philalethes, be he, who he would, could not be offended at an inquiry after truth, provided it were conducted with decency and good manners; it was however added, that I had in two places cenſured doctrines, which, if I ſuppoſed to be the opinions of Philalethes, I muſt have miſapprehended him.

NOW tho' I was fully ſatisfied, that the errors condemned in thoſe two paſſages, which he referred to, had really been advanced by Philalethes, as in the ſequel of this controverſy did more plainly appear; yet without any diſpute or heſitation I immediately expunged them, and publiſhed the remaining part in the Republick of Letters for October 1735, as an account of my book on Sir Iſaac Newton's method of fluxions, and of prime and ultimate ratios.

TO this Philalethes anſwered in the following month, and I again replied, till five papers were ſucceſſively written in this controverſy, that is, three by me, and two by him. And all this time ſo very deſirous was I on my part of avoiding all irritating circumſtances, that, though I had written my laſt paper of the two with ſuch circumſpection, that I thought even the moſt intimate friend, with whom I had differed in opinion, eſpecially if he had conſented to the diſpute, could not be offended with it; yet, as I had there treated ſomewhat diffuſely on the uncertainty and confuſion, which would be introduced into mathematical inquiries, if the opinions of Philalethes

were

were received, I choſe on reviewing it to expunge near twenty pages, rather than risk the diſpleaſure of that gentleman by diſcuſſing a topic, that might poſſibly have proved diſtaſteful to him.

BUT alas this caution proved altogether fruitleſs; for Philalethes in his reply, part of which was publiſhed the July following, and the reſt in the ſucceeding month, runs out into the moſt extravagant heats of paſſion; not with the common complaints of indecency or ill manners, but charging me with diſhoneſtly writing againſt the convictions of my own judgment; for thus that reply begins.

> “*TO enter into controverſy for any other cauſe than that of truth is beneath the character of a man of letters; but to perſiſt in diſpute, when he is clearly ſhewn his error, when his mind is under ſtrong and invincible convictions, were much more unworthy of ſuch a character: Flatter himſelf, as he may, whoever acts ſo diſingenuous a part, can never paſs for what he would appear to be, &c.*”

AFTER this proem there follows a detail of certain rules, by which the advocate for truth is to be diſtinguiſh'd foom this other obſtinate diſputant, that thus wickedly violates the impulſes of his own conſcience: And theſe rules are ſuch, as in the courſe of his reply he accuſes me of having broke; in ſhort I am charged in this diſcourſe with defending my own opinions, after I am fully ſatisfied of their falſhood, and with wilfully miſquoting, and miſrepreſenting thoſe of my antagoniſt.

AFTER

AFTER ſo groſs and unprovoked an abuſe, which was the more odious too, as it ſeemed in great meaſure to be owing to the tenderneſs, with which I had treated this author, I ſhould ſurely have been acquitted of any breach of decency, if without any farther reſerve I had ſharply expoſed his ignorance in the ſubjects, he had attempted. But I choſe, if poſſible, to avoid the ridicule of quareling on a matter of meer ſpeculation with one, whom I believed to be an acquaintance.

BESIDES I imputed this ſudden ſally of paſſion to an unexpected rebuff, he had met with in the diſpute. I had in my laſt paper produced his definition of naſcent increments (a), *upon which I had grounded great part of my cenſure of his repreſentation of the doctrine in queſtion, and to which hitherto I had only alluded. This definition is as follows,* (b) " *A naſcent increment " is an increment juſt beginning to exiſt from " nothing, or juſt beginning to be generated, but " not yet arrived at any aſſignable magnitude, " how ſmall ſoever." As I imagined his embarraſment at the review of theſe unintelligible, and conſequently indefenſible words, or ſome other difficulty, he found himſelf under in the diſpute, had occaſion'd this emotion, which he would ſoon wiſh for an opportunity to recover himſelf from, within three days after the publication of the firſt part of the reply I again requeſted my Friend to ſpeak to Dr. Jurin, and to repreſent to him the inconveniencies, that would ariſe from the perſeverance of Philalethes in his raſh and groundleſs calumny.*

(a) *Republick of Letters for April* 1736. *p.* 320.

(b) *The Minute Mathematician, p.* 19.

calumny. My Friend accordingly went to Dr. Jurin, and carried with him an anſwer to ſo much of Philalethes's paper, as was then publiſhed, and told the Doctor, that he came to propoſe to him a method, that might prevent the controverſy betwixt me and Philalethes from degenerating into a paſſionate perſonal altercation, to which the late behaviour of Philalethes ſeemed to precipitate it; that he had in his hand my reply to every particular urged by Philalethes in the part of his anſwer, which was then publiſhed, drawn up in the very ſhort time elapſed ſince its publication, which might be reaſonably ſuppoſed ſo ſhort as to exclude all ſuſpicion of my apprehending any difficulty in defending myſelf; that therefore, if Dr. Jurin thought it expedient, my paper ſhould be given to a certain gentleman, to whoſe impartiality and knowledge of the ſubject in debate no exception could be taken on either ſide; and that if, when that gentleman had peruſed it, he ſhould believe, I had in any inſtance changed my opinion from my firſt entering into this diſpute, I did then promiſe to ſubmit patiently and without reply to any cenſures of unfairneſs and diſhoneſty, that Philalethes either had already paſſed on me, or ſhould hereafter think proper to load me with; but if on the contrary, when he had read my defence, he ſhould be of opinion (without entering into the merits of the queſtion in debate) that I had not yet varied in my judgment from thoſe principles, on which I originally begun the controverſy, it would then be but common juſtice, that Philalethes ſhould moderate the remaining part of his performance, which was to come out the following month, and ſhould for the future refrain from perſonal reflections; ſince ſurely a charge of ſo criminal a nature, as that of wilfully mis-

miſrepreſenting both one's own ſentiments, and the ſentiments of others, ſhould never be admitted, whilſt any one impartial perſon acquainted with the ſubject could diſbelieve it.

BUT this propoſal was rejected, and inſtead of preventing Philalethes from expoſing himſelf farther, render'd him more inſolent and untractable; for it was immediately given out, that my friends had confeſſed me to have been foiled in the argument; and were now only ſollicitous to ſupport me from the charge of unfairneſs. Nay ſo far was his weak imagination inflated by theſe condeſcentions, that he thence concluded me to be entirely at his mercy, and therefore in the ſecond part of his paper indulged himſelf in ſtill more undiſguiſed defamation; for I having mentioned Sir Iſaac Newton's modeſty with applauſe, he tells his reader (a), *he ſhould be glad, he had as much to ſay for me, adding a few lines lower "That Sir Iſaac Newton's modeſty is ſurpriſing to Mr. Robins, I do not wonder."*

FROM the account, I have here given of the various ſteps, I have fruitleſly taken, to divert Dr. Jurin, who was then generally underſtood to write under the name of Philalethes, from continuing his abuſive exercitations, the reader will not wonder, if I reſolved for the future to treat him with that freedom, which his unskilfulneſs authoriſed, and his petulancy made neceſſary; ſince after the ſelf-ſufficiency, he had aſſumed meerly from my caution of offending him, and the rude returns, he had made to my profer'd civilities; I ſhould have thought it a blameable want of reſentment, had I ſtill continued to ſtudy palliative phraſes

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(a) *Republick of Letters, July* 1736. *p.* 177.

phraſes in my farther animadverſions on his errors.

BUT tho' in the proſecution of that controverſy, and ſince in my Remarks on the Eſſay upon diſtinct and indiſtinct viſion, I have cenſured freely and without any circumlocution the errors and ignorance of his performances; yet the reader will be very much deceived, if he believes, that my retaliation has in any manner kept pace with the abuſive inſults of Philalethes. For whilſt I confined myſelf to the diſcuſſion of the matter of ſcience in debate, only defending my own opinions, or expoſing the incapacity of my antagoniſt, without engaging in any other perſonal diſquiſition; he on the other hand ſuppoſing himſelf ſcreened under the vizard of Philalethes launched out into the wildeſt ſtrains of ſcurrility, beſides running away into ſuch ſubjects and circumſtances, as could only be intended to lead the reader from the merits of the diſpute.

FROM this time a very conſiderable part of his performances, both in what he writ againſt me, and alſo in his ſubſequent controverſy with another perſon, which I ſhall have occaſion to mention farther hereafter, were made up of a ſtrange medly of Latin verſes, conſiſting of ſuch an awkward attempt at parody, and ſuch an abſurd application of thoſe verſes, as cannot poſſibly give the reader any other amuſement, than to ſpeculate, how it could come about, that a perſon, who diſcovered ſuch an abſolute want of all taſte, ſhould have read the ancient poets enough to be able to remember ſo many lines, as are here thus pedantically applied.

IN

IN one of theſe ſcholaſtick proluſions the author has employed two pages (a) *in the deſcription of a Lord Mayor's feaſt, and of the various evacuations occaſioned by the gluttonous exceſſes, with which he is pleaſed to reproach thoſe publick feſtivals* (b). *Nay he has condeſcended to grace his poetry with a very minute and circumſtantial account, of all that paſſes at a bog-houſe, beginning it with this decent line,*

Ad ſellam tremulo deſcendit clune puella.

AND in theſe his ſacrificings to the muſes I am introduced from time to time by the name either of Robnus, Robinus, Robnides, Robnigenus, or Robinia Mater, where under the various characters of a wounded ſoldier, a ſhipwreck'd mariner, a ghoſt, a nightingale, and an old woman, I am made to own my crimes, to bewail my diſgraces, and to accuſe my hard fate, that had thus match'd me with ſo ſuperior an adverſary.

AND the better to compleat this wretched mummery, and that his proſe might not be ill ſuited to the impertinence of his verſe, he has continually interſperſed it with ridiculous impotent attempts at jocularity, and dull pedantic allegories; one of which contains a very complaiſant dialogue between the goddeſs of indolence and the author (c), *and in another conſiſting of near three pages we are told of the wonderful effects wrought on the author by certain greens, which his Bedmaker had brought*

(a) *Works of the Learned for March* 1737. *p.* 235.

(b) *Ibid. p.* 237.

(c) *Republick of Letters for December* 1736. *Append. pag.* 33, 34.

brought into his chamber in order to deck his windows at Chriſtmas (a).

NOW had theſe moſtly extravagancies been produced with any degree of freedom and unconſtrainedneſs, one might have been tempted to have excuſed them by ſuppoſing, that ſome forward ſchool boy had a little too haſtily aſſumed the character of an author, and had endeavoured to ſet off his ſmall ſmatterings of ſcience by interlacing them with his exerciſe verſes. But were the gravity of the author unknown to us, the ſtiff and cumbrous hand, that perpetually appears in theſe performances, would preclude any ſuch excuſe. No, theſe frozen conceits, and labour'd abſurdities bear not the leaſt marks of levity, or puerility, but are evidently the ſtrained efforts of a lethargic imagination awkwardly toiling under the teazing impulſes of a moſt demure and ſolemn vanity.

IT need not indeed ſurprize us, that our author ſhould exert his utmoſt faculties in the invention of theſe groteſque decorations, ſince he is ſo weak as to value himſelf greatly upon them, boaſting of the advantage, they gave him in the controverſy, telling us (b), *that*

" A verſe may catch him, who a ſermon flies;

" *that if we will not take a little pains to diverſify*
" *and enliven the ſubject, l'egayer un peu, it will be*
" *impoſſible to keep up the reader's attention;" and that he hopes " his verſes may now and then give*
" *a little refreſhment and diverſion amidſt ſo much,*
" *and*

(a) *Republick of Letters, Nov.* 1736. *Append. p.* 65.

(b) *Ibid. Dec.* 1736. *Append. p.* 7.

" and ſo intenſe thinking;" and again, that he intends them " pour faire bonne bouche," as he is pleaſed to expreſs it.

NAY ſo ſtrangely was he infatuated with theſe prepoſſeſſions, that he was vain enough in expreſs terms to profeſs himſelf a moſt peſtilent wit (fœnum habet in cornu) (a); *and could not refrain from making his friend, Dr. Smith, a party, and confeſſing, that he was ſupported in theſe ſcurrilities by having his applauſe, declaring, that ſo long as he could make Dr. Smith laugh, he cared not whom he abuſed* (b).

INDEED this proneneſs to abuſe, even in thoſe inſtances, where no provocation had been previouſly given him, was moſt compleatly verified by the whole tenor of his conduct; for, not contented with the ſpecimens he had furniſhed of this diſpoſition in his behaviour to me, he reſolved to exerciſe it with ſtill more glaring circumſtances of folly and arrogance on a gentleman, who was no otherwiſe intituled to theſe ventilations of this author's ſpleen, than by the friendſhip, with which he was known to honour me.

PHILALETHES to be conſiſtent with himſelf muſt needs take part with the leſs knowing among the modern mathematicians, who have ſpoke ſlightly of the ancient writers; and an expreſſion in the former editions of Sir Iſaac Newton's Prin-

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(a) *Republick of Letters, for Decemb.* 1736. *Append. p.* 18.

(b) —— *Dummodo riſum Fabriadæ excutiat, non cuiquam illudere parcet. Ibid.*

Principia, which might be underſtood, as ſubſcribing in ſome ſort to this opinion, being in the laſt edition of that work changed (a); *he makes no ſcruple invidiouſly to inſinuate* (b), *that Dr. Pemberton, who under the inſpection of Sir Iſaac Newton had taken care of this laſt edition, had unfairly procured this paſſage to be thus changed.*

NOW the very ſuppoſition of a fraud in that affair was ſufficiently ridiculous; ſince, as Sir Iſaac Newton himſelf reviewed each ſheet, as it came from the preſs, it would have been little leſs than madneſs in Dr. Pemberton to have endeavoured to deceive him, eſpecially in a circumſtance, where no inducement for ſuch a proceeding could reaſonably be ſuppoſed to intervene; for the alteration was of ſo ſmall account, that I had never obſerved it, till Philalethes produced it with his infamous comment. However, notwithſtanding the improbability of this charge, Dr. Pemberton thought proper to take notice of it publickly, and did endeavour to obviate the ſuggeſtions of this author by aſſuring him, that the alteration in queſtion had been inſerted by the expreſs direction of Sir Iſaac Newton. Nay Dr. Pemberton went farther, and told him the reaſon, Sir Iſaac Newton himſelf gave for this change (c).

BUT Philalethes on this reply of Dr. Pemberton, inſtead of taking to himſelf the ſhame, with which his groundleſs calumny ought to have covered

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(a) *In Princ. p.* 37. *l.* 8. *Edit.* 3. *the word longas is ſubſtituted in the room of perplexas, the term made uſe of in the former editions.*

(b) *Republick of Letters for Aug.* 1736. *p.* 178.

(c) *Ibid. for Sept.* 1736. *Append. p.* 37.

covered him, perſiſted in it, for he allowed, that Sir Iſaac Newton might indeed conſent to the alteration in queſtion; but he ſtill maintained, that in ſo doing he had acted contrary to his own opinion, and that he had with reluctance acquieſced meerly to avoid the preſſing importunity of Dr. Pemberton, of whoſe obſtinacy he endeavours to give us a moſt formidable idea (a).

NOW ſurely Philalethes ought to have been able to have produced very good evidence, that Sir Iſaac Newton was thus weakly overawed by Dr. Pemberton, before he had ventured to advance ſo ſtrange an accuſation; ſince it was ſcarcely credible, that one, who was doubtleſs well pleaſed with being ſelected by Sir Iſaac Newton for the honourable office of aſſiſting in the reprinting of this work, ſhould have injudiciouſly riſqued the favourable opinion of that great man by endeavouring thus tyrannically to over-rule him even in the very beginning.

BUT we find, that inſtead of producing teſtimonies or vouchers of any kind in ſupport of this wild and wicked imputation, the whole proof is at laſt reſolved into the conjectures of Philalethes, and his certain perſuaſion, that Sir Iſaac Newton's opinion muſt have been different, from what the change in this amended paſſage would evince it to be. Thus a gentleman well known to Philalethes, who had never given him any ſubject of offence, was to be accuſed of a crime little ſhort of forgery on no better foundation, than the ſuſpicions and ſurmizes of this cloudy headed author, and his vain belief, that he could in every inſtance

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(a) *Republick of Letters for Dec.* 1736. *Append. p.* 13, 14.

ſtance infallibly aſſign the preciſe and accurate limits of Sir Iſaac Newton's opinions; a preſumption, that muſt ſurely appear highly ridiculous after the ſpecimens, we have given in this preface, of Philalethes's egregious abſurdity.

NOR did this affair reſt here; for Dr. Pemberton having engaged in diſpute with Philalethes in conſequence of this unexpected attack on his character, he alſo experienced in the courſe of it the full meaſure of this author's ſcurrility, and indiſcriminate abuſe. Nay Philalethes in one place expreſly owns, that he had " treated Dr. Pem-" berton in ſuch a manner, with ſo many marks " of contempt, as few men of letters have been " treated with" (a)*; and Dr. Pemberton having taken notice of this monſtrous confeſſion, and wondred, that his antagoniſt ſhould not be aſhamed thus openly to avow his crime, Philalethes with his uſual effronterie tells him in his reply, that he is ſo far from being aſhamed of it, that he makes a merit of it* (b).

AFTER the account, I have now finiſhed, of the perſonal inſults, which this author, though unprovoked, has dealt about on every hand, the complaint of Dr. Jurin againſt me for having treated him with ill manners muſt appear highly unreaſonable; for as that gentleman acquieſced under the imputation of being the author of the papers bearing the name of Philalethes Cantabrigienſis, and as I have in ſo many inſtances been attacked by the virulent defamations of thoſe abuſive pieces,

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(a) *Works of the Learned for May* 1737. *p.* 394.

(b) *Ibid. for July* 1737. *p.* 78.

pieces, ſurely he can have no pretence to think himſelf hardly uſed by me, whatever retaliation I have already taken, or ſhall hereafter think proper to take, eſpecially as I avow my name, and therefore am anſwerable to the public for the reaſonableneſs of my conduct; whereas he was doubtleſs hurried into his ridiculous behaviour by the mean conſideration, that, being, as he preſumed, concealed, neither the contempt nor deteſtation of the public could poſſibly reach him.

HOWEVER, tho' I had undoubtedly a right to have entered with the greateſt freedom into the moſt perſonal diſcuſſion of Dr. Jurin's behaviour; yet, as what I have hitherto conteſted with him, have been points of ſcience only, I thought, I owed more reſpect to the reader, than was conſiſtent with the interruption, which the ſubject muſt have received from the mixture of theſe uſeleſs altercations; and therefore, before I engaged in this preface, which I have undertaken only in vindication of my own character, I have ſhewn no other reſentment, than cenſuring with the ſame truth, though with leſs ceremony than I ſhould otherwiſe have done, the groſs and matchleſs ignorance of this author.

DR. Jurin is ſo ſenſible, how ill his behaviour becomes him, that though he ſees, the diſguiſe cannot be worn any longer; yet he could not help ſtill attempting to keep it on by the mean ſubterfuge of asking only, if it is not poſſible, that I may have pitched upon the wrong perſons (a). *But tho' the public ſhould have been all this while miſtaken in this particular, yet the errors of Dr. Jurin, which I have cenſured, are ſtill the ſame errors; and if my animadverting on them is ſo very grievous to this gentleman, as his great anger ſeems to import, he can only blame his unaccountable conduct in ſuffering himſelf to be ſo univerſally thought the author of all the preceeding ſcurrilities and abuſe, without ſo much as undeceiving even his friends. However this ſo lame an evaſion, when he denies in ex-*

(a) *Reply in the Preface.*

plicit terms Dr. Smith to have written under the name of Philalethes, directly fixes thoſe papers, and all the rudeneſs and folly of them upon himſelf.

FROM the puerile miſtakes in mathematics, that ſo often occurred in Philalethes's writings, whatever I might apprehend at firſt, I did not myſelf long continue in the belief, that Dr. Smith was the author of them, tho' I often heard, he applauded them among his acquaintance; but when I with aſtoniſhment ſaw the many blunders, he had committed in his Compleat Syſtem of Optics, I had leſs reaſon to diſcredit, what has been ſuſpected, that at leaſt Philalethes had received aſſiſtance from that gentleman, and as Philalethes himſelf had publiſhed to the world, that Dr. Smith was his great encourager, in all his abuſes and ſcurrilities (a), *I thought myſelf at liberty to mention, what was ſurmiſed of that gentleman, which has now drawn from Dr. Jurin ſuch a declaration, as fixes the offence upon the true author, and will undeceive thoſe few, who out of their good wiſhes towards him were willing the ſhame might rather fall upon ſomebody leſs known to them.*

DR. Jurin ſeems to conceive, that he has made a very ſhrewd reflection upon my want of manners (b), *by ſaying, that he and his friend Dr. Smith are of opinion, that I underſtand as much of good manners, as I do of Sir Iſaac Newton's firſt Lemma. As to the Lemma, I think, I have ſufficiently defended my ſenſe of it. How well Dr. Smith underſtands it, appears in my* (c) *Remarks on his book. The other's interpretation has been proved to be downright nonſenſe and inconſiſtent with itſelf till he was abſolutely ſilenced after the moſt obſtinate and tenacious contention in the defence of it. As to what judges of manners this couple of friends are, may be known from the writings of Philalethes. But I think it no breach of good manners to deſpiſe and treat with contempt, whoever is mean enough to ſpeak a man fair to his face, and at the ſame time rail at him under a diſguiſe.*

(a) *See above, p.* xiii.

(b) *Rep. in the Preface.*

(c) *Pag.* 69.

DR. Jurin's ſaying, (a) *that "with whatever groſs "ill manners I have been treated, it cannot be denied, "that I have executed the Lex Talionis in its full extent," can only be accounted for from the ſame want of apprehenſion, as has given riſe to the reſt of his behaviour; for the ſuppoſing, that to charge him with errors in a matter of ſcience, will be conſider'd as a breach of good manners, equal to his accuſing another of wilful prevarication and lying, is a degree of arrogance, that I cannot imagine, even this gentleman could be knowingly guilty of.*

IN regard to Dr. Smith I am not ſenſible, that I have cenſured his book in other terms, than what I might have uſed of any man unknown to me, of whoſe performance I had ſo little eſteem. But if he wiſhes, I had expreſſed myſelf leſs freely, he can only blame his choleric friend for making him a party to his injurious behaviour.

THIS gentleman is pleaſed to ſay (b), *that I ſhall at length be aſhamed of my remarks on Dr. Smith's book. But I am not like to repent of cautioning the publick againſt doctrines ſubverſive of great part of the ſcience of optics, till I am ſhewn to have cenſured wrongfully, which neither Dr. Smith himſelf, nor this gentleman on his behalf has yet attempted to do. But theſe writers ſeem happy in their want of diſcernment, which keeps them in countenance, by their not perceiving the wretched reaſoning that all their productions abound with.*

I SHALL now conclude with a word upon Dr. Jurin's laſt effort towards ſupporting himſelf, when he endeavours to engage on his ſide at leaſt ſuch, as are not judges of the controverſy, by labouring to perſuade them from the reſentment I have expreſſed of his abuſes, that I have the worſt of the argument; in the true and genuine ſpirit of Philalethes expatiating upon that trite tale to be found in common jeſt-books of a cobler frequenting univerſity diſputes, and judging who was worſted by ſeeing, who firſt gave way to paſſion (c).

BUT

(a) *Rep. in the Preface.* (b) *Rep. p.* 54. (c) *Rep. at the end.*

BUT is it not wonderful, that this gentleman ſhould thus advance poſitions ſo evidently deſtructive to his own cauſe? If he, that yields to paſſion firſt, has undoubtedly the worſt of the argument, how can Philalethes be defended, who has been obſtinately perſiſting in a controverſy even years, after he has afforded us thoſe proofs of his being worſted, which Dr. Jurin here conſiders as deciſive?

HOWEVER, though it be a ſtrong preſumption, that thoſe, who firſt give a looſe to paſſion in a diſpute, do it to conceal their want of argument; yet the ſame ſuſpicions cannot take place againſt him, who with ſome warmth endeavours to repel injurious attacks from his antagoniſt. But not to enter farther into the diſquiſition of what degree of reſentment, the numerous affronts, I have received, might reaſonably authoriſe; I can aſſure this gentleman, that the temper, with which I have examined and cenſured his writings, hath been very far removed from that perturbation of mind and vehemence, which is uſually ſtiled anger. It is indeed conſtantly the wiſh of all, who are under the influence of that weak paſſion, that the object of their diſpleaſure might ſuffer the ſame uneaſineſs, as they feel within themſelves. But our caſes are too different. The ſame weakneſs, which prompted this gentleman to aim at a character in a ſcience, where, with the leaſt reflection he muſt have been conſcious of his inſufficiency, occaſions him to bear his diſappointment in this froward and childiſh manner. As for me however the abuſes, I have received, might on ſome other occaſion juſtify no ſmall ſhare of reſentment; yet this miſtaken man may reſt ſatisfied, that thoſe harmleſs efforts of his wrath can never move any ſerious indignation in the perſon, they are thrown out againſt, while they only render their author the ſubject of pity among his friends, and expoſe him to be laughed at by every indifferent looker on.

A
FULL CONFUTATION
OF
Dr. JURIN's Reply.

DR. Jurin in the firſt paragraph of his Reply, finds fault with my having ranged the contents of his Eſſay under four general heads; for he tells us, that it might as well have been aſſerted to conſiſt of ſix, eight, or nine; and that he himſelf never thought of any ſuch diviſion.

THIS, I ſuppoſe, is ſaid to inſinuate, that I have animadverted on a few particulars only, and left uncenſured the greateſt part of the Eſſay; whereas, I believe, every reader will ſee, that I have made a juſt diviſion of it; and, if the Doctor is in earneſt, in what he here ſays, it will only ſhew, that the general ſcheme and order of this performance was never well digeſted in his own thoughts.

HE next ſuppoſes, that (*a*) I " having little to " object againſt what I have called the two firſt general heads, which contain much the greater and " moſt conſiderable part of the Eſſay; and having, " as I imagin'd, effectually refuted ſome paſſages " of the third, and half a dozen lines at the end

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(*a*) Pag. 1. lin. 7.

“ of the fourth, I was willing to make theſe
“ victories appear as important, as I could.”

BUT, in oppoſition to what is here aſſerted, I do inſiſt, that my exceptions do affect all the principal matters advanc'd in that treatiſe. I have cenſured the firſt head, as conſiſting for the greater part of propoſitions confeſſed, even by this gentleman himſelf, to be erroneous; and tho' I have been ſhorter in my remarks on the ſecond, becauſe of the anatomical diſquiſitions, with which it is blended, yet I have ſaid enough, even on that head, to ſhew, that the ſcheme there advanced was altogether chimerical. I have irrefragably made out, that the third part is the moſt undeniable proof of the conſummate unſkilfulneſs of the author, even in the commoneſt conceptions of the doctrine, he has there attempted to diſplay; and I have ſhewn with regard to the fourth head, that the principal motive, which induced this gentleman to dwell ſo long upon it, was the moſt raſh and groundleſs imputation on Sir Iſaac Newton.

TO be more particular, I have aſſerted in my Remarks (*a*), that Dr. Jurin had treated the ſimple caſe of a white object ſeen on a black ground imperfectly, he ſuppoſing, that the light is uniformly ſpread thro' his circles of diſſipation, whereas he confeſſes, at art. 220, that the denſity of the light is different in different parts of theſe circles.

To this the Doctor anſwers, that (*b*) in writing upon an intricate ſubject the way to be underſtood

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(*a*) Remarks, pag. 93. line 1. (*b*) Reply, pag. 3. line 5.

derſtood is to put the ſimpler caſes firſt, and, that before he enter'd on thoſe caſes, where it was neceſſary to conſider that inequality, he did expreſly mention it.

How evaſive is this? The pretence of conſidering the ſimpleſt caſes of the ſubject firſt will no ways excuſe him; for he has conſider'd neither the ſimple, nor the compound caſes of the ſubject in hand; he has ſpent his labour in forming a number of propoſitions relating to the appearance of objects, when ſeen indiſtinctly, and for the baſis of many of theſe propoſitions he has aſſum'd a falſe principle, by which means the greateſt part of his determinations are alſo falſe; he afterwards does indeeed own, that the ſuppoſition, he has proceeded on, is not true, but does not attempt to rectify the propoſitions, he had ſo elaborately reared on this fallacious foundation.

I HAD inform'd this gentleman in my Remarks (*a*), that it was not owing to any of thoſe idle reaſons aſſign'd by him, that the diameter of the moon obſerv'd before the invention of teleſcopes differed leſs from the more recent obſervations, than what on his ſcheme it ought to do; but that it was owing to the ſights the ancients uſed. This is now in ſome meaſure confeſſed (*b*); but this omiſſion, as it is ſtiled, is endeavoured to be extenuated, by ſhewing, that in another place of the Eſſay the advantages of ſuch ſights are taken notice of: That is, he is excuſable, though he made no mention of theſe ſights in the caſe, where they actually produce the effect in queſtion, but inſtead

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(*a*) Pag. 93. line 5. (*b*) Rep. pag. 4. line 23.

inſtead thereof aſſign'd falſe reaſons for the fact; becauſe he afterwards ſpoke of theſe ſights in juſtification of the obſervations of Hevelius, who did not truſt to the ancient form of theſe ſights, but uſed others of a more artificial compoſition, to which I have ſhewn the accuracy of his obſervations to be owing.

I HAD obſerv'd in my Remarks (*a*), that Dr. Jurin was miſtaken in aſſigning the cauſe of the dark ſpot, ſeen, when a black circumference of a circle is viewed on a white ground with a certain degree of indiſtinctneſs; for that the ſpaces GCHE and IFKD only, in the figure, I have there drawn, are dark, but all without thoſe ſpaces are more or leſs illuminated.

To this the Doctor replies (*b*), that he is much at a loſs to know what I mean, that in ſome inſtances my aſſertion (that the ſpaces GCHE and IFKD are deprived of light) is not true, and that in other inſtances it is inconcluſive.

IF he does not underſtand me, it is certainly not my fault. I had very expreſly deſcribed thoſe ſpaces GCHE, IFKD by ſuppoſing CE and DF to be the diſtinct images of a tranſverſe ſection of the circle; and in this caſe any geometer can inform him, that theſe ſpaces will be as much deprived of light, as the original object it ſelf: and, ſince all without theſe ſpaces, is more or leſs illuminated, and no part of theſe ſpaces can be ever found in the center of any indiſtinct image of the circle; it follows, that the black ſpot, which does ſometimes appear

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(*a*) Pag. 93. line 18.

(*b*) Rep. pag. 5. line 12.

pear in that center, is not produced from the cauſe aſſign'd by the Doctor: and to this original error he has now added another ſtill more glaring by aſſerting (*a*), that in certain circumſtances theſe ſpaces will not be deprived of light; whereas they are always ſo, and the circumſtances urged by him are wholly foreign to the purpoſe, they not affecting the truth or falſhood of this ſuppoſition.

I HAD alſo obſerved in my Remarks (*b*), that this appearance of the black ſpot was not truly deſcribed; for that the middle darkneſs did not extend into a ſimple penumbra gradually vaniſhing, but that the ſhade was compoſed of a number of different images, and appeared uniform, only when thoſe images became ſo numerous or dilated, as not to be eaſily diſtinguiſhed.

THE Doctor anſwers (*c*), that if the ſhade appears uniform, and he has deſcrib'd it as uniform, then he apprehends the appearance is truly deſcribed.

BUT by what occurs above, I have aſſerted, that it is not uniform, unleſs in a particular inſtance; either then I am miſtaken, or the Doctor has erred in his deſcription, in making this uniform appearance general; and, as he has elſewhere expoſed his want of judgment by not attending more minutely to this appearance, it may not be amiſs more particularly to deſcribe it.

A SMALL black circumference drawn on white paper and viewed either at too great or too ſmall a diſtance

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(*a*) Rep. pag. 5. line 24.

(*b*) Pag. 94. line 10.

(*c*) Rep. pag. 6. line 14.

diſtance to be ſeen diſtinctly will to all the eyes, that I have conſulted, appear like many circles not concentric but variouſly interſecting each other: and this multiform appearance, tho' viewed with one eye, will not be conſtant and ſteady, but will ſeem to have ſome fluctuation, and will uſually appear ſomewhat different to each eye. Now from this multiplication of the image, which takes place in all indiſtinct viſion, ariſes not only the central ſpot, we have juſt now mentioned (when the inner parts of theſe circles unite) but alſo thoſe other appearances, which Dr. Jurin has handled in the latter part of his Eſſay, and in the accounting for which he has ſo unneceſſarily and unſkilfully dragged in Sir Iſaac Newton's optical diſcoveries.

This multiplied image of the circle formed by circles not concentric cannot ariſe, but from inequalities in the eye analogous to thoſe, that occur in a common multiplying glaſs.

And indeed after our author had ſo far attended to the appearance of this circle, as to have diſcovered, that in ſome ſituations the axis of the eye would be ſo much deprived of light, as to produce a ſpot intenſely black (*a*), he muſt have been ſtrangely deficient in the ſcience of optics not to have perceiv'd, that this circumſtance was an inconteſtable proof, that ſome parts of the eye concerned in the refraction of the light were more or leſs diſtorted. For, if the ſurfaces of all the humours kept the regularity of their ſhape, having one common axis with the axis of the eye; it would then be impoſſible, that a dark border of

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(*a*) Rep. pag 5. at the bottom.

of any breadth whatever encompaſſing a bright circle held before the eye in its axis, could in any caſe divert the light, which came from the middle of that circle, out of the axis of the eye. As then the appearance of this central ſpot could not be owing to the various combinations of penumbras ſo diffuſely expatiated on by this gentleman, I did in my Remarks expreſs my opinion to be, that it aroſe from ſome corrugation or inequality of ſurface, to which that part in the eye, that is changed for the different diſtances of objects, is ſubject in its extreme tenſion either way. And that this ſuppoſition of the cauſe of it cannot be far diſtant from the truth, we ſhall ſhew hereafter in its proper place.

BUT this gentleman, ignorant of the real appearance of this phenomenon, and unskilled in optics, tells us (*a*), that the multiplication of the image often happens, when in all probability the eye is not at all ſtrained; which, from what has been ſaid, appears to be plainly impoſſible. He is alſo pleaſed with a ſtrain of buffoonery, the moſt frigid, that ever dulneſs coined, to cenſure my ſuppoſition of its ariſing from ſome corrugation in ſome part of the eye, as (*b*) "a mere "hypotheſis ariſing from ſome corrugation in "ſome part of my brains."

I MUST here deſire this gentleman alſo to reconſider the caſe of the dark angle, which he has drawn (fig. 36, 37. of his Eſſay) as appearing like two angles lying partly upon one another, and where they ſo meet looking blackeſt. This repreſentation does indeed bear a reſemblance

(*a*) Rep. pag. 7. line 17. (*b*) Rep. pag. 7. line 1.

blance to the real appearance; but from the cauſe, he has aſſigned for the phænomenon, the ſpace between the two appearances, which he repreſents as fully illuminated, ought alſo to have been obſcured, for the penumbra muſt encompaſs the object. This indeed is only one caſe of that multiple appearance of indiſtinct objects, which he has afterwards attempted to ſolve on other principles, and its being inſerted here can, I think, be no otherwiſe accounted for, than by ſuppoſing this part of the eſſay to be writ, before the other had been in his thoughts; and being ſure of the fact he could not ſee the error in the cauſe, he has here aſſigned for it, while he had no conception of any other.

Dr. Jurin had in his Eſſay endeavoured to defend Hevelius from the cenſures of Dr. Hook, who has inſinuated, that, as few eyes can diſtinguiſh a ſpot viewed under a leſs angle than a minute; therefore all aſtronomical obſervations made without teleſcopic ſights could not be generally relied on, but for the minutes only, whatever was the radius of the inſtrument; and from hence he would conclude, that the accuracy in obſervation pretended to by Hevelius could not be truſted to. Now Dr. Jurin attempted to obviate theſe exceptions by ſuppoſing Hevelius on account of his age and long practice in aſtronomical obſervations, to have acquired a particular length of ſight. And on this defence I had taken notice in my Remarks (*a*), that Dr. Jurin underſtood not the method of obſervation practiſed by Hevelius; ſince from the deſcription of his ſights, which I have there given, and which were firſt brought

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(*a*) Rem. pag. 95. line 1.

brought in uſe by Tycho Brahe, it appears, that the accuracy of his obſervations would be little influenced by the longer or ſhorter ſight of the obſerver; and that therefore the mere imagination of an extraordinary length of ſight, which Hevelius might acquire from age and long exerciſe, is a very trifling defence.

To this the Doctor replies (*a*), that it is ſufficient for his purpoſe, that the obſervation is influenced by the longer or ſhorter ſight of the perſon, tho' but little.

THAT is, tho' Dr. Jurin has defended the accuracy of Hevelius's obſervations ſolely from a circumſtance, to which very ſmall part of that accuracy could be owing, and knew not the principal cauſe, from whence that accuracy did ariſe, yet his defence is neither trifling nor ignorant.

I AM next accuſed (*b*) of falſely charging him with acknowledging himſelf to have been formerly of the ſingular opinion of Monſ. de la Hire, that the eye had no power of ſuiting itſelf at will to the different diſtances of objects, with an inſinuation of not having a due regard to what, I thought the truth. For he ſays, his words are directly contrary to M. de la Hire.

Now in the paſſage, I referred to (*c*), this gentleman concludes from his own trial of M. de la Hire's experiment, that the neareſt diſtance, at which with one eye he could ſee any of the objects diſtinctly, was 40 inches; but indeed he alſo tells us, that ſometimes 50, 60, and even 90 inches

were

(*a*) Rep. p. 7. l. 27.

(*b*) Rep. p. 8. l. 3.

(*c*) Eſſay §. 105.

were the diſtance, at which he ſaw the object diſtinctly, and this he now pretends is directly contrary to M. de la Hire; which is not ſo: it is indeed quite contrary to the common opinion, but very conſiſtent with M. de la Hire's. What he ſays, that the neareſt diſtance for diſtinct viſion he found to be forty inches in his eye, is irreconcileable with the common doctrine; for that always ſuppoſes objects to be ſeen perfectly diſtinct at that diſtance, where a perſon will chuſe to hold them to view them with greateſt advantage; now no one with a ſound eye holds an object at more than a yard's diſtance for that purpoſe: and he does not ſay, that he could ſee the object diſtinctly in thoſe other diſtances at pleaſure, but only, that his eye was not always the ſame at different times; and tho' ſuch accidental differences in the eye are not mentioned by M. de la Hire, yet they are in no wiſe contrary to his opinion: he himſelf ſuppoſes, that age will create ſuch a difference. The only thing contrary to his doctrine is, that the eye can at its choice ſuit itſelf to the different diſtances of objects. Moreover the phraſe, in which this gentleman mentions Dr. Porterfield's experiments, is exactly the ſame, as it muſt have been, had he himſelf been before of a different opinion, and different from what, I believe, every one will think, it would have been, had he intended only to ſay, that Dr. Porterfield had confirmed him in the opinion he had before (*a*). To which let me farther

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(*a*) Eſſay §. 105. "But "as my eyes are now conſiderably decayed with regard to their power of ſeeing diſtinctly at ſmall "diſtances, and as Dr. Porterfield's experiments ſeem "to be better contrived, and "were more methodically "made than mine, I fully "acquieſce in the proof he "has given, that the eye has "a power of altering its conformation ſo as to ſee objects perfectly diſtinct at "different diſtances.

farther add, that the minute diſcrimination expatiated upon by our author between two ſorts of diſtinct viſion, the perfect and the leſs perfect (*a*), is of little or no uſe in any, but M. de la Hire's opinion, and by him is at large inſiſted upon for reconciling his doctrine with the degree of diſtinctneſs, with which common objects are ſeen at all moderate diſtances.

DR. Jurin has advanced in his Eſſay (*b*) a new hypotheſis relating to the manner, in which the conformation of the eye is changed in viewing ſucceſſively objects placed at different diſtances; and ſuppoſes this change to be principally effected by a certain ſuppoſititious muſcle, which has never been ſeen by any anatomiſt.

NOW upon this I obſerved in my Remarks (*c*), that before we conſidered this his new ſcheme any farther, we ought to aſk the anatomiſts, "whether a muſcle ſtrong enough to "keep the eye tenſe can be inviſible."

BUT Dr. Jurin in his Reply (*d*) tells me, that "the uvea, and that part of the uvea, "by whoſe action he ſuppoſes the eye to be "kept tenſe, is not inviſible." And then he ſubjoins, "Is this gentleman acquainted with "no anatomiſt, &c. that can give him better "information."

BESIDES the abſurdity of this reaſoning, that the exiſtence merely of a part is a ſufficient proof of there being any muſcle, a theoriſt may fancy exiſting

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(*a*) In the firſt 13 articles of the Eſſay.

(*b*) §. 127, &c.

(*c*) Pag. 96. l. 25.

(*d*) Pag. 9. l. 3.

iſting in it, can the reader, after what is here ſaid, imagine, that Dr. Jurin himſelf had acknowledged this muſcle never to have been ſeen; yet theſe are his words. "It will perhaps be ob-"jected to me, that the exiſtence of this ſuppo-"ſed greater muſcular ring (*the muſcle in queſtion*) "has not yet been proved by ocular demonſtra-"tion. I anſwer neither has the exiſtence of the "leſſer muſcular ring been yet proved in the "ſame manner (*a*)."

In my Remarks (*b*) I had obſerved, that Dr. Smith choſe to be very conciſe in regard to the fits of eaſy reflection and tranſmiſſion of light; and I particularly took notice, that his compleat Syſtem of Opticks was on this argument ſo very deficient, that theſe alternate fits of reflection and tranſmiſſion were not ſo much as mentioned by name, where he has undertaken to exhibit Sir Iſaac Newton's diſcoveries in optics. And I thereupon refer the reader to the 6th, 7th, and 8th chapters of his firſt book.

This aſſertion is cenſured by Dr. Jurin in his Reply (*c*), as diſingenuous, and as intended to miſlead the reader; and a dreaming dialogue is introduced between him and me more effectually to expoſe my ſophiſtical views. The whole of that tedious diſquiſition conſiſts of the following particulars.

1. That my reference to the 6th, 7th, and 8th chapters of the 1ſt book is unfair; ſince Dr. Smith did not in thoſe chapters undertake to exhibit all Sir Iſaac Newton's diſcoveries in optics,

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(*a*) Eſſay §. 129. toward the end.

(*b*) Pag. 97. l. 2.

(*c*) Pag. 10. l. 3.

optics, but has exhibited ſome in one place, and ſome in another.

2. THAT, as theſe fits are mentioned in another part of the book (art. 507.) where mention is alſo made of an optical diſcovery of Sir Iſaac Newton's; therefore my references will not excuſe me from the charge of having been guilty of a blameable artifice.

3. THAT tho' Dr. Smith has no where explained theſe fits (which is at laſt confeſſed) yet this omiſſion is no deficiency; for (*a*) "he has "in one of thoſe chapters mention'd the effect "of them, and for the fits themſelves, which "are the cauſe of that effect, he has referred his "readers to Sir Iſaac Newton's optics, a book "in every body's hands, where they are largely "treated of, and that was ſufficient for his de"ſign. As he intended to deduce nothing "from theſe fits, he might in this reſpect be "as conciſe as he pleaſed."

4. IT is alſo inſinuated, that I had a more ſecret meaning in reſerve, which I am deſired bona fide to declare.

To the firſt of theſe articles I reply, that I referred to thoſe chapters, becauſe their titles (*b*) and ſubject did, as it were, enforce Mr. Profeſſor to have there mentioned them; that it is ſo true, that in theſe chapters he more particularly intended

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(*a*) Rep. p. 12. l. 9.

(*b*) Their titles are, concerning the origine and cauſe of colours; concerning the cauſe of refraction, reflection, inflection and emiſſion of light; concerning tranſparency, opacity, and colours in bodies.

ed to exhibit Sir Iſaac Newton's principal diſcoveries in optics, that the whole of them (containing ſcarcely any thing, but literal tranſcripts from that author) is by many times more in quantity, than all the other quotations from him in the reſt of the book amounts to; that however, if the other parts of Dr. Smith's book had abounded with explanations of Sir Iſaac Newton's optical diſcoveries, yet if they were ſuch, as were unconnected with the nature of theſe fits, the general charge of omitting them might be fairly proved by referring the reader to thoſe chapters, where, without ſome care and contrivance to have avoided it, the mention and explanation of them muſt neceſſarily have occurred.

On the ſecond head I muſt obſerve, that my aſſertion (that theſe fits are not ſo much as mentioned by name, where Dr. Smith has undertaken to exhibit Sir Iſaac Newton's diſcoveries in optics) ſtands unimpeach'd either of falſhood or unfairneſs, notwithſtanding the mention of them in art. 507. here referred to, for that article is a tranſcript from a piece of Dr. Pemberton's publiſhed in the Philoſophical Tranſactions (*a*), wherein he accounts for a particular appearance in the rain-bow, that had been before obſerved by the reverend Dr. Langwith, and, as the ſolution there given depends on two principles diſcovered by Sir Iſaac Newton, one of which is this diſpoſition of each ray to be alternately reflected or tranſmitted, Dr. Pemberton has accordingly mentioned theſe diſcoveries, not with a view of explaining them to thoſe, who knew them not before, but ſolely to prepare the ſkilful

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(*a*) Numb. 375.

ſkilful reader for the deductions, he immediately draws from them; the purpoſe, he had in view, being to account for the phenomenon obſerved by Dr. Langwith, not to exhibit any of Sir Iſaac Newton's diſcoveries in optics.

BUT it is really wonderful, that our author can thus gravely deſcant on this head, when he has himſelf confeſſed, that Dr. Smith has not in his compleat Syſtem of Optics treated of theſe fits; for after this conceſſion ſhould he prove, that they were accidentally mentioned a thouſand times in that work, nay, in the very chapters, I have referred to, yet it would not excuſe his friend; my expreſſion would then indeed be exceptionable, but my cenſure would ſtill be juſt, and the compleat Syſtem of Optics would ſtill be extremely deficient in not containing an explication of this wondrous and important diſcovery. And this brings us to the examination of Dr. Jurin's excuſe for this ſtrange neglect (*a*), "that as to thoſe fits he has in one of thoſe chap-"ters (thoſe I had referred to) mentioned the "effect of them, and for the fits themſelves, "which are the cauſe of that effect, he has re-"ferred his readers to Sir Iſaac Newton's optics, "a book in almoſt every body's hands, where "they are largely treated of, and that was ſuffi-"cient for his deſign. As he intended to deduce "nothing from theſe fits, he might in this reſpect "be as conciſe as he pleaſed."

IT muſt indeed be confeſſed, that Dr. Smith would have dealt kindly by his readers, had he referred them to other authors for information on many of thoſe heads, he has himſelf endeavoured to

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(*a*) Rep. p. 12. l. 9.

to explain; he would thereby have ſaved them the labour of ſtruggling with his inaccuracies, paralogiſms and blunders; but thoſe, who are of this opinion, think, he would have done better never to have written any thing on this ſubject. However, when he had intitled his work a compleat Syſtem of Optics, and had ſo filled it with literal tranſcripts from other books, particularly from Sir Iſaac Newton's Optics, he ought not to have neglected ſo material a part, as is the conſideration of theſe fits, eſpecially, ſince by his friends confeſſion he has mentioned the effects of them; for by what art of ſpeech can that treatiſe be ſtiled compleat, where effects are treated of without once mentioning the known cauſe, to which they are owing.

As to the demand made by Dr. Jurin (*a*), that I ſhould bona fide declare, what was my ſecret intention in this charge againſt Dr. Smith's book, I anſwer, that I believed not my intention to have been a ſecret; but, ſince he is pleaſed to conceive it as ſuch, I ſhall without heſitation confeſs to him, that I imagined this affected omiſſion was owing to an averſeneſs in Dr. Smith to engage in a ſubject, he perceived himſelf not to underſtand. What other conſtruction could be put on his ſingular conduct? That he underſtood not the ſubject, the monſtrous errors on this head in Dr. Jurin's Eſſay, which Dr. Smith has publickly approved of, fully ſhew.

We are now come to the conſideration of Dr. Jurin's defence to that more important charge, which we have brought againſt him in the

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(*a*) Rep. p. 12. l. 1.

the Remarks on his Eſſay (*a*), viz. that he has endeavoured to apply and improve a part of Sir Iſaac Newton's doctrine relating to the alternate fits of tranſmiſſion and reflection of light, when at the ſame time he appears to have been ſo totally unſkilled in it, that he was incapable of deſcribing even the firſt rudiments of it.

THAT the reader may the better judge of the validity of this gentleman's defence, we will here give an abſtract of the reaſons, we did formerly urge in ſupport of our accuſation.

DR. Jurin had obſerved, that lines and many other objects ſeen by indiſtinct viſion appear to be multiplied, and to account for this appearance he makes uſe of that property of light diſcovered by Sir Iſaac Newton, that each ray is alternately diſpoſed at equal intervals to be tranſmitted and reflected.

AND to ſhew, how the multiplication of the image muſt ariſe from this diſpoſition of the rays, he ſuppoſes a pencil of rays diverging from a point in the axis of the eye, and falling on the cornea, where, ſince the interval between the point of emiſſion, and the point of incidence on the cornea grows longer, as the rays fall more diſtant from the middle point of the cornea, he thence concludes, that if the ray, which falls on the middle of the cornea be in a fit of tranſmiſſion, the rays falling at a ſmall interval from that middle point will be in a fit of reflection, and thoſe removed ſtill ſomething more will be again in a fit of tranſmiſſion; ſo that the

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(*a*) Pag 110. at the bottom.

whole ſurface of the cornea would be divided by circles concentric to the axis into zones, in one of which if the light was tranſmitted, in the next it would be reflected, and in the next tranſmitted again, &c. and therefore if a plane were held behind the cornea not paſſing thro' that point, to which the rays converge by their refraction at the cornea, the image of the lucid point formed on this plane would conſiſt of luminous and dark circles alternately recurring. And upon this principle he endeavours to account for the multiplication of objects, when they are ſeen indiſtinctly.

Now we have ſhewn in our Remarks, that this ſolution could only be imagined by one ſtrangely ignorant of the doctrine, he attempted to apply: for had he but known, that the rays of different colours had their intervals of tranſmiſſion and reflection of different lengths, he muſt have ſeen, that the whole of his reaſoning would have immediately vaniſhed; ſince from hence it follows, that after the interval of 9 or 10 fits from the luminous point, wherever the rays of one ſpecies are in a fit of tranſmiſſion, the rays of many other ſpecies are in a fit of reflection; and therefore there would be no perceptible difference between the light reflected at one point of the cornea, and another.

It was alſo objected to him (*a*), that he had not only groſsly miſtaken Sir Iſaac Newton in ſuppoſing the length of the fits of tranſmiſſion and reflection to be the ſame in all kinds of rays; but that he appeared ignorant of the conſequences even of this his abſurd conception of this doctrine:

(*a*) Rem. p. 108.

doctrine: ſince by a computation founded on the ſuppoſition, that the rays of every ſpecies have the length of their fits the ſame, it is ſhewn in the Remarks, that the image of a luminous ſpot could not even in the moſt favourable ſituation be ſurrounded with rings, that would be at all viſible.

AGAIN, Dr. Jurin in order to account for the different quantities of light, which are reflected from different bodies, ſuppoſes in his Eſſay, that, when light falls on any pellucid ſurface, the extent of the fit of reflection will bear a different proportion to the interval of the fits according to the quantity of light, which is reflected at that ſurface; the extent of the fit of reflection being greateſt, when moſt light is reflected.

IN oppoſition to this hypotheſis I urged in my Remarks *(a)*, that Sir Iſaac Newton without any regard to the quantity of light reflected made in general the length of the fit of tranſmiſſion equal at leaſt to the length of the fit of reflection, and that this near equality between the extent of the two fits was abſolutely neceſſary to the producing the appearances obſerved in the colours of thin plates.

LASTLY Dr. Jurin at the end of his Eſſay has very diffuſely treated on the dazzling, to which the eye is ſubject from the immediate contraſt of light and darkneſs, and its diſpoſition, after it has been ſtrongly impreſt with either of theſe ſenſations, to excite of its ſelf the

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(a) Pag. 105. at the bottom.

oppoſite ſenſation. And he tells us, that he ſhould not have been thus particular on this head; but that he could not help ſuſpecting, that even the great Sir Iſaac Newton himſelf was once led into a miſtake by a like appearance; " at leaſt, adds the Doctor, I cannot find, " that he has any where accounted for the extraordinary ſtrength of the ring of light next " the central dark ſpot ſpoken of in Obſ. 23. " Part I. of his ſecond book of Optics, nor do " I ſee any way of accounting for it, but from " ſome ſuch conſideration."

In reply to this charge againſt Sir Iſaac Newton I obſerved (*a*), that if it were a miſtake, it were indeed a very great one; ſince Sir Iſaac ſeems ſo aſſured of the contrary, that he ranges the colour of white metals (the ſtrongeſt white we are acquainted with) under the ſame claſs with the white of this firſt lucid ring; that, however it were, he could not have been deceived by the contraſt of the neighbouring black ſpot, becauſe he tells us, that he compared two bubbles together blown at different times, on one of which there appeared the white, that (on the bubble) preceded all the colours, and on the other the white, that followed them all, and that this laſt white would often quite vaniſh, before the black ſpot appeared; that indeed it was not wonderful, that Dr. Jurin ſhould not know how to account for it after the ſpecimens, he had before given us of his utter ignorance of this ſubject, but that this appearance was eaſily to be accounted for; it following immediately from the method laid down by Sir Iſaac Newton for inveſti-

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(*a*) Remarks, pag. 109. at the bottom.

inveſtigating the ſeveral colours exhibited by thin plates; for from thence it appears, that at a thickneſs of the plate adapted to produce this colour all the light, that enters it, arrives at its farther ſurface in a fit of reflection, a circumſtance, that occurs not in any other thickneſs whatever.

THESE are the three articles, with which I have charged him in the laſt fifteen pages of my Remarks; where many of the arguments, which I have here but mentioned, are more diffuſely explained. And it is a defence againſt this charge, that is attempted in the laſt forty-four pages of Dr. Jurin's Reply from page 13 to the end; and to the examination of this defence we will now proceed.

IT opens page 13 and 14 with a quotation from my Remarks, which by being falſely printed is indeed ungrammatical; but if this gentleman will pleaſe to correct it by inſerting the words *each ray* after the word *freely* in the 7th line, he may ſoon ſee, on how groundleſs a foundation he has here given a looſe to his pointleſs impertinence.

DR. Jurin then chooſes to begin with defending himſelf from the ſecond article, I had charged him with, and having quoted paſſages from my Remarks, in which I aſſert, that Sir Iſaac Newton makes the fit of reflection equal in length to the fit of tranſmiſſion, and that this equality between them was the effect of mature deliberation, and neceſſary towards producing the appearances deſcribed by him; the Doctor re-

replies (*a*), that not a tittle of this is true, that Sir Iſaac Newton no where makes the fit of reflection in the ſame ray equal in length to the fit of tranſmiſſion; with near a page more of the ſame poſitive caſt; defying me to ſhew any paſſage in Sir Iſaac Newton's Optics, in which the equality between the extent of theſe fits is aſſigned; boaſting likewiſe of certain experiments, whereby he had endeavoured to determine the proportion between the light reflected, and the light tranſmitted at its paſſage between air and glaſs (*b*), as if they had been experiments any way conducive to confirm his hypotheſis; whereas they relate ſolely to that fact, and do not pretend to ſhew any thing concerning the cauſe, why different tranſparent ſubſtances bounded by the ſame medium reflect different quantities of light.

Now in anſwer to this confident challenge it is ſufficient to exhibit the following quotation from Sir Iſaac Newton's Opticks, B. 2. prop. 12. The propoſition itſelf is thus ſtated, "Every ray of light in its paſſage thro' any "refracting ſurface is put into a certain tranſient "conſtitution or ſtate, which in the progreſs of "the ray returns at equal intervals, and diſpoſes "the ray at every return to be eaſily tranſmitted thro' the next refracting ſurface, and "between the returns to be eaſily reflected by "it." And this is afterwards thus explained. "This action or diſpoſition in its propaga- "tion intermits and returns by equal inter- "vals,

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(*a*) Rep. pag. 14. l. 10.

(*b*) But now this precipitate cenſor without either trying or queſtioning my experiment, &c. Rep. p. 23. l. 12.

“ vals, becauſe in all its progreſs it inclines “ the ray at one diſtance from the firſt ſurface to be reflected by the ſecond, at another to be tranſmitted by it, and that by “ equal intervals for innumerable viciſſitudes. “ And becauſe the ray is diſpoſed to reflection “ at the diſtances 1, 3, 5, 7, 9, &c. and to “ tranſmiſſion at the diſtances 0, 2, 4, 6, 8, 10, “ &c. (for its tranſmiſſion thro’ the firſt ſurface is at the diſtance 0, and it is tranſmitted “ thro’ both together, if their diſtance be infinitely little, or much leſs than 1) the diſpoſition to be tranſmitted at the diſtances “ 2, 4, 6, 8, 10, &c. is to be accounted a “ return of the ſame diſpoſition, which the “ ray firſt had at the diſtance 0, that is, at “ its tranſmiſſion through the firſt refracting “ ſurface. All which is the thing I would “ prove.” This propoſition is laid down univerſally of every ray of light without any exception; and the method directed to find the ſubſequent fits of reflection and tranſmiſſion is to multiply continually the interval between the firſt fit of reflection and the ſurface, without the leaſt intimation, that this method is to be varied in any ray whatever. Now if every ray, which paſſes the ſurface A H in the 6th figure of the ſecond book of Sir Iſaac Newton’s Optics be not in a diſpoſition to be reflected, when it arrives at the line 2 K, but that the firſt fit of reflection is in ſome confined to a nearer diſtance from the ſurface, and in ſome falls beyond; if theſe intervals be multiplied according to the method of this propoſition, the regularity of the following fits will be totally deſtroyed.

For inſtance, if any ray, inſtead of being in the middle of its fit of reflection at the line 2 K, ſhould arrive at the middle of its fit at $\frac{2}{3}$ of the interval between A H and 2 K (and in Dr. Jurin's hypotheſis this is ſuppoſed to happen to great numbers of rays tranſmitted at A H). Then according to the rule here laid down by Sir Iſaac Newton the next fit of reflection would in ſuch a ray be at twice the diſtance of the line 2 K from A H, that is, it would fall in that place, where we know all the rays are diſpoſed to be moſt freely tranſmitted.

Now then if every ray, which paſſes the ſurface, be in a fit of reflection at the line 2 K, as Sir Iſaac Newton here repreſents it; ſince the diſpoſition to reflection is deſcribed, as extended on each ſide this line as far, as half the diſtance of this line from the ſurface, and in conſequence thereof the ſpaces between 1 I, 3 L, and between 5 M, 7 O, &c. the limits, within which rays may be reflected, are repreſented to be equal to the ſpaces between 3 L, 5 M, and between 7 O, 9 P, &c. the limits, within which rays may be tranſmitted; Sir Iſaac Newton has indubitably ſuppoſed that equality between the extent of the fit of tranſmiſſion and reflection, which Dr. Jurin has ſo raſhly denied.

Hence appears the juſtneſs of that ſuppoſition of ours, which Dr. Jurin has ſo abuſively cenſured (*a*). I mean the taking the diſtances between the lines 1 I, 3 L, &c. to denote

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(*a*) Rep. pag. 25. l. 22. But who in the name of blindneſs can it be, that tells Mr. Robins the ſpace 13, is the extent of the fit of reflection in a ſingle ray? &c.

denote the extent of a fit of reflection in a ſingle ray. For ſince Sir Iſaac Newton does in this propoſition deſcribe each ray, as coming to the middle of its fit of reflection at the line 2 K, he has by this means determined every ray to be put into the middle of a fit of tranſmiſſion at the ſurface A H, and conſequently the ſpaces between 1 3 and I L, between 5 7 and N O, &c. where the ſubſequent reflections take place, are the extents of the fits of reflection of the rays tranſmitted at A H.

AND as by this propoſition I knew, that Sir Iſaac Newton ſuppoſed the middle of the fits of reflection of every ray of the ſame ſpecies to be at the ſame diſtances from the ſurface, the light enters; I ſhewed upon this principle, that Dr. Jurin's rule for confining and varying the extent of theſe fits was abſolutely inconſiſtent with the phænomena in the colours of thin tranſparent plates.

IN regard to this hypotheſis, where all the light in a fit of reflection is ſuppoſed to be actually reflected, it appears from the 6th figure of the ſecond book of Sir Iſaac Newton's Optics, that, when light has paſſed through any great number of fits of reflection and tranſmiſſion between its entrance into the firſt ſurface of any tranſparent medium and its arrival at the next reflecting ſurface, the ſpaces 1 I L 3, 5 M O 7, 9 P R 11, &c. in which Sir Iſaac Newton repreſents the rays of light as in fits of reflection, muſt be contracted in breadth in proportion to the quantity of light reflected; that is, in all common objects, where the light incident upon them, has paſſed for a great length through the air, the extent of the fit of reflection ought to bear that proportion to the whole interval between the fits, which Dr. Jurin has aſſigned. But though this ſuppoſition accounts for the quantity of light reflected in common from objects; yet in my Remarks I have ſhewn at large, that thus confining the extent of the fits of reflection, will alter the ſpecies of the colours of thin plates, cauſing them to be otherwiſe compounded, than Sir Iſaac Newton has repreſented them to be, and not to be always the ſame; in particular, that in the ſmall extent aſſigned by Dr. Jurin for the fits of reflection in glaſs and water, theſe colours would be extremely more ſimple.

AND all, I have there ſaid, is very obvious upon Sir Iſaac Newton's principle, that all the rays of the ſame ſpecies are in their fits of reflection at the ſame diſtances from the ſurface they have entered. But Dr. Jurin ſeeing the abſurdity of his ſcheme upon this footing, proceeds here on another model with much abuſive exultation to accommodate this hypotheſis to thoſe appearances, which I had aſſerted could only be accounted for by an equality between the extent of the fits of reflection and tranſmiſſion. And to this end he has premiſed a long diſcuſſion of his hypotheſis (*a*), ſpinning it out into eleven propoſitions, beſides other additional ſuppoſitions, the whole of which are ſtill founded on the erroneous poſition, that, when light falls on the ſurface of a pellucid medium, all the light, that is in a fit of reflection, is reflected, and all the light, that is in a fit of tranſmiſſion, is tranſmitted; whereas Sir Iſaac Newton plainly diſtinguiſhes between a ray's being

(*a*) Rep. p. 17. and following.

being in a fit of reflection, and its being actually reflected, in his 14th propoſition of the ſecond book, which is thus laid down, " Thoſe ſurfaces of tranſparent bodies, which, if the ray " be in a fit of refraction, do refract it moſt " ſtrongly, if the ray be in a fit of reflection, do " reflect it moſt eaſily." Here it is evidently ſuppoſed, that a ray may be in a fit of reflection, and yet not be in fact reflected; for if every ray, which is in a fit of reflection at any ſurface, be certainly reflected, no one ſurface can be ſaid to reflect the rays, when in a fit of reflection, more eaſily than another.

BUT this hypotheſis is now alſo irreconcileable with Sir Iſaac Newton's deductions from the phænomena. In the 9th propoſition of the Reply (*a*) it is laid down, that, " when homogeneal light falls either perpendi" cularly, or with any given angle of incidence " upon the plain ſurface of a tranſparent ſubſtance, " the quantity of light reflected from that ſurface " is to the whole incident light, as the extent of " the fit of reflection in any one ray is to the whole " interval between two ſubſequent fits of eaſy " tranſmiſſion in the ſame ray." To prove from hence his original hypotheſis, that in tranſparent ſubſtances illuminated by the common light of the day the extent of the fit of reflection in each of the incident rays bears to the whole interval between the fits in that ray the ſame proportion, as the whole quantity of light reflected bears to the whole of the light incident upon the tranſparent ſubſtance, Dr. Jurin was obliged to aſſume in his 10th and 11th propoſition (*b*), that ſuch tranſparent ſubſtances indifferently

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(*a*) Reply p. 21. (*b*) Rep. p. 21, 22.

rently tranſmit or reflect all the ſpecies of light, not one more than another. Now this is not the caſe of ſuch ſurfaces; for, when the light having paſſed through the air falls upon any tranſparent body, or after entering the firſt ſurface of a body of any thickneſs arrives at the ſecond, according to Sir Iſaac Newton's doctrine, as repreſented in the 6th figure of the ſecond book of his Optics, near half the ſpecies of light will be wholly tranſmitted.

Moreover this gentleman has now made his hypotheſis even inconſiſtent with itſelf. After aſſerting in the 9th propoſition (*a*) of homogeneal light in general, that as many rays will meet the ſurface, upon which they are incident, in any one point of the interval between the fits, as in any other; in page 26. l. 14. of the Reply it is ſaid, "that the rays "of the extreme violet (the rays our author "chuſes to inſtance in) which are tranſmitted "through the tranſparent ſubſtance (ſurface he "means) at A, cannot all be in the middle, "or be put into the middle of the fit of eaſy "tranſmiſſion at that point A:" and in page 27. l. 20. it is notwithſtanding aſſerted, that "yet the greater part of them will there be "put either into the middle of that fit, or "very near it." Here is not a word ſaid to ſhew, how one may happen, though the other cannot; but the aſſertion is made in the ſpirit of the looſeſt coiners of hypotheſes, who think themſelves at liberty to multiply ſuppoſitions, juſt as ſhall be thought expedient for the ſupport of a favourite ſcheme, without concerning themſelves in the leaſt about any connection between

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(*a*) Rep. pag. 21. l. 10.

between them. However in conſequence of the ſecond aſſertion it is ſhewn (l. 22. of p. 27.) that the greater part of the rays " will be in the mid-" dle of their fit of eaſy reflection, or very near " it, at the thickneſs A 2 :" and the ſame he muſt intend to be underſtood at all the following diſtances, A 6, A 10, A 14, &c. Thus, when rays of light have entered the ſurface of any tranſparent medium, and are come to the next ſurface, where this medium is terminated by ſome other body, it is laid down, that at this ſurface (that is at its incidence on the other body) in all thoſe ſpecies of rays, in which the diſtance of the ſecond ſurface from the firſt is equal to the diſtance denoted by any one of the lines 2 K, 6 N, 10 Q, 14 T, &c. the greater part of thoſe rays will be in the middle, or very near the middle of a fit of reflection, when they fall on that ſurface. And this will always be the caſe of ſome ſpecies, nay in all common objects, upon which the light falls, it will happen to very many. This is a direct contradiction to the firſt aſſertion above cited (*a*) from the 9th propoſition of homogeneal light in general, that as many rays will meet the ſurface, upon which they are incident, in any one point of the interval between the fits, as in any other.

THIS aſſertion in the 9th propoſition is laid down for the proof of that propoſition, which propoſition is the baſis of the whole hypotheſis, upon the validity of which it entirely depends. Yet here, as ſoon as he comes to apply his hypotheſis to the phænomena, he found himſelf obliged to contradict this foundation

(*a*) Pag. 28.

dation of his ſcheme, and thereby has himſelf abſolutely ſubverted it.

All this is more than ſufficient againſt this laboured hypotheſis. It were indeed enough to object to it, that it is an hypotheſis, which for that reaſon only ought upon Sir Iſaac Newton's principles to be rejected; eſpecially as it is nowiſe neceſſary toward accounting for any of the phænomena. For if we may allow place to conjecture, the ſame phænomena may be more eaſily accounted for, only by ſuppoſing, that tho' every ray of the ſame ſpecies be alternately diſpoſed to reflection and tranſmiſſion at the ſame diſtances from the ſurface, it enters; yet, that all the rays of the ſame ſpecies ſucceeding each other may not receive their diſpoſitions to reflection equally ſtrong (*a*). By this means, where the reflecting power is weakeſt, thoſe rays only, which at the lines 2 K, 6 N, 10 Q, &c. are moſt ſtrongly diſpoſed to receive the influence of that power, will be reflected, and the reſt, tho' in their fit of reflection, will paſs on; but where the reflecting power is ſtronger, more rays will be reflected. In like manner, tho' all the rays are ſimilarly diſpoſed towards tranſmiſſion and reflection from the ſurface A H to the line 2 K, and onwards; yet thoſe rays, in which the diſpoſition to reflection is weakeſt, will ceaſe to be operated on by the reflecting

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(*a*) For example, if the rays of light are put into theſe alternate diſpoſitions by exciting at the refracting ſurface a vibratory motion in the refracting medium, as Sir Iſaac Newton propoſes; it is not difficult to conceive, how the vibrations, that are excited by the different rays of the ſame ſpecies, which enter the ſurface in ſucceſſion, may in ſome of the rays be more vigorous than in others.

reflecting power at a leſs diſtance from the lines 2 K, 6 N, 10 Q, &c. than thoſe, which are more diſpoſed to be influenced by that power.

BUT to proceed, our author reſts not here, wandering from the point in hand to ſhew us, how little care he has even to this time taken to inform himſelf of the ſubject, on which he will write. In ſhewing the inconſiſtency of his hypotheſis with Sir Iſaac Newton's experiments (*a*) I had deſcribed the colours in the order, they appeared on the bubble of water; denominating each colour to be of the fifth or ſeventh order, according as they were the fifth or ſeventh in ſucceſſion, as the bubble grew thinner: and hence this gentleman takes occaſion to ſpend near two pages in dull pedantic cenſure (*b*) on this method of proceeding; cloſing his puerile reflections with theſe two aſſertions (*c*), that Sir Iſaac Newton conſtantly numbers the ſeries of colours from the black ſpot outwards, and that I (*d*) without giving any notice of my intention, have numbered them the contrary way.

NOW both theſe aſſertions are abſolutely falſe. For Sir Iſaac Newton in his ſecond Book, Obſ. 18. has deſcribed the colours in the ſame manner, I have done, calling each colour 1ſt, 2d, 3d, 4th, &c. according to the order, in which they aroſe on the bubble; and contrary to the order, in which they are number'd from the black ſpot outwards: And I had in the beginning of the paſſage here referred to given notice, that I alſo reckoned them, as they aroſe on

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(*a*) Rem. pag. 106. l. 3.

(*b*) Rep. p. 30.

(*c*) Rep. p. 31. l. 14.

(*d*) Rep. p. 31. l. 16.

on the bubble, and had added this reaſon for my doing ſo, that in this order they were very diſtinctly deſcribed in the Optics with all their concomitant circumſtances; that deſcription being by Sir Iſaac Newton conſidered as much more accurate, than any he could deduce from the colours of the air thinned between two glaſſes.

HAVING thus evinced the fallacy of Dr. Jurin's defence to the ſecond head of accuſation, as we have recounted them above; we muſt next follow him in his Reply on the third head relating to the brightneſs of the ring ſurrounding the central ſpot; ſince he for reaſons, which he beſt knows, has choſen to anſwer theſe two articles firſt, leaving our firſt charge to be conſidered laſt.

WE have recited above, that, in oppoſition to what he in his Eſſay had advanced on this article, we in the Remarks (*a*) informed him, that Sir Iſaac Newton in his account of the ſuperior brightneſs of that ring could not be impoſed on by the neighbourhood of the central ſpot, as Dr. Jurin had ſuppoſed, becauſe its degree of light was eſtimated, when no ſuch central ſpot exiſted, and that, when Dr. Jurin ſuppoſed no other account could be given of the extraordinary ſtrength of light of this ring, than what aroſe from ſome ſuch fallacy, he betrayed his want of knowledge in this ſubject; ſince it was the immediate conſequence of the theory eſtabliſhed by Sir Iſaac Newton, that the light of this ring ſhould be much ſtronger than that of any of the other rings, or than the white

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(*a*) Rem. pag. 109.

white light reflected from the parts of the thin medium or plate without the rings.

OUR author in reply (*a*) to theſe exceptions tells me, that I did not underſtand him; and now pretends (*b*), that his difficulty aroſe from conceiving Sir Iſaac Newton to have ſuppoſed the brightneſs of this ring to be of ſo extraordinary a kind, as to equal that of white metals. But this excuſe confeſſes as great an error, as that I cenſured, and more evidently proves, how little he had conſidered the ſubject; for no one tolerably acquainted with the ſecond book of his Optics could ever have conceived, that Sir Iſaac Newton ſuppoſed the light of that firſt ring, either when formed by the plate of air, or by the bubble of water, to be as ſtrong, as the light reflected by white metals; nothing being more diſtinctly inculcated in that treatiſe, than that the colours of the ſame order are more or leſs vivid, as they are formed by plates of different denſities. Nor was that careful reviſal (*c*), Dr. Jurin now pretends to have made, of what that great man has in different places ſaid upon this ſubject, at all neceſſary for diſcovering his miſtake. Sir Iſaac Newton in the very place, where he compares the light of this ring with that of white metals, directly ſays the contrary to this gentleman's imagination in theſe words (*d*). "And as the "white of the firſt order is the ſtrongeſt, which "can be made by plates of tranſparent ſub-"ſtances, ſo it ought to be ſtronger in the denſer "ſub-

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(*a*) Rep. p. 33. l. 17.

(*b*) Ibid. p. 34. l. 11.

(*c*) Ibid. l. 16.

(*d*) Optics B. II. part 3. prop. 7. p. 233.

" ſubſtances of metals, than in the rarer of air, " water, and glaſs."

WHAT reaſon Dr. Jurin has to charge me with being as ignorant on this head, as he confeſſes himſelf to have been, I cannot imagine. If he does really believe ſo, it can only ariſe from his conceiving, that every perſon in acquiring the ſimpleſt truths muſt run through the ſame tedious round of blunders as himſelf.

THE proſecution of this matter in page 35, depending altogether on that chimerical hypotheſis of our author, which we have ſo often confuted; it were treſpaſſing on the reader's patience to allot it a more particular diſcuſſion in this place.

WE now therefore come to what we made the firſt article of our accuſation, but what Dr. Jurin has choſe to reply to laſt; he does however confeſs it to be the principal, and moſt important point.

WITH regard to the attempts of this author to account for the double and triple appearance of objects ſeen indiſtinctly by means of the fits of tranſmiſſion and reflection we have ſhewn him in the Remarks (*a*), that even on his own fallacious principle, making the extent of theſe fits the ſame in rays of every colour, ſuppoſing them to fall on the cornea in the ſituation the moſt favourable to his hypotheſis, and the rings formed by this means to be received on the retina placed much more out of the poſition requiſite for diſtinct viſion, than what he preſcribes; yet with all theſe conceſſions the rings formed on the retina

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(*a*) Page 108.

retina will be ſo ſmall as to be utterly imperceptible; and that therefore, ſince he ſuppoſes theſe rings to be viſible, though under circumſtances much leſs advantageous than thoſe, we have mentioned, he had not only ſtrangely erred in his hypotheſis, but appeared ignorant of the conſequences even of thoſe abſurd conceptions, he had framed to himſelf on this ſubject.

Now in reply to this imputation (*a*) he denies, that he ſuppoſes the rings to be viſible, as they are tranſmitted from the cornea, becauſe he had mentioned their being again divided at both ſurfaces of the cryſtalline, and it was by this ſecond and third modulation of them, that he conceived they might be ſometimes ſo augmented, as to become viſible.

BUT I have ſhewn, that the number of theſe rings formed by the cornea within the compaſs of his circle of diſſipation muſt exceed 600 (*b*), and the cryſtalline to produce the appearance in queſtion ought to obliterate all but 5, or 6, or a leſs number at regular diſtances; whereas the ſubdiviſion of them, which he aſcribes to the cryſtalline (*c*), would really multiply them, and would render them leſs perceptible; conſequently my ſuppoſing them to be continued without interruption from the cornea to the retina was a circumſtance altogether in his favour: and therefore, ſince he founds his defence ſolely on the augmentation, which he aſſerts, the breadths of theſe rings may ſometimes receive at the ſurfaces of the cryſtalline, and ſince in fact theſe

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(*a*) Rep. p. 36. at the bottom.

(*b*) Rem. p. 108. l. 5.

(*c*) Eſſay §. 215, 216.

rings never can be augmented in breadth at thoſe ſurfaces, but would be always diminiſhed, the whole of our animadverſion muſt be confeſſed to remain in full force againſt him.

He next takes into conſideration, what he is pleaſed to call my ſecond objection (*a*), which, he ſays, he is ſenſible may at firſt ſight appear plauſible to many readers; and yet in the very next lines (*b*) he declares himſelf greatly at a loſs to know certainly, what the objection is.

Now, if he were really as ignorant of the meaning of this objection, as he pretends, how came he to be ſo ſenſible of its plauſibility? for what reaſon too does he ſtile it the ſecond objection? It is both the principal one, and occurs in my Remarks many pages before the other, which he has been pleaſed to conſider as the firſt.

I had in my Remarks (*c*) given a particular account of the fits of tranſmiſſion and reflection, and had ſhewn in conſequence of the theory delivered by Sir Iſaac Newton, that the diverſity in the colours and in the degrees of brightneſs of very thin plates ariſing from the various lengths of thoſe fits in rays of different kinds would vaniſh, as ſoon as the plates were ſo thick as to have the fits repeated any conſiderable number of times within their ſubſtance; and to the end of this diſquiſition I had added (*d*), that for this reaſon "in all thick plates, and conſequently in "all the common appearances, the light reflect- "ed from every point of either ſurface of colour- "leſs

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(*a*) Rep. p. 38. l. 3.

(*b*) Ibid. l. 10.

(*c*) Pag. 97. and following.

(*d*) Rem. p. 103. l. 13.

" leſs tranſparent ſubſtances, whatever be the " figure of their ſuperficies, is not only always " white, but alſo the ſame in quantity of light, " the ſame proportion of the whole light arriving at each point of ſuch ſurfaces in fits of " eaſy reflection;" and to this obſervation I immediately ſubjoined " Hence it appears, how " abſolutely this gentleman has erred in this " whole matter, &c." The error I imputed to him (as immediately follows in the ſame place) was his ſuppoſing, when a pencil of rays fell upon the cornea, that all thoſe rays, which fell on ſome parts of the cornea, would be in fits of reflection, and would be accordingly reflected, whilſt all thoſe, that fell on other parts, would be in fits of tranſmiſſion, and would be therefore tranſmitted.

AND ſurely this charge does moſt obviouſly follow from the preceding paſſage quoted by us above; ſince, as we have there obſerved, that after a very few repetitions of theſe fits the rays, that are at any point of incidence, in a fit of reflection, will be blended with an equal number of rays of other ſpecies, that are at the ſame point in a fit of tranſmiſſion; the deſcribing, as our author has done art. 209, the rays iſſuing from a lucid point to be in ſome incidences all in fits of reflection, and in other incidences all in fits of tranſmiſſion did moſt irrefragably prove his total ignorance of this ſcience.

BUT notwithſtanding this evident connection between the paſſage, we have quoted above, and the accuſation we ſubjoined in the next paragraph, our author with his uſual ribaldry endeavours

vours to repreſent the word *Hence*, with which that paragraph begins, as enigmatical, myſterious and quite ſurpaſſing his comprehenſion; and yet four or five pages afterwards (*a*) he confeſſes the fallacy of his ſcheme upon my very reaſons againſt it, which he there repeats from the place, he here pretends to be ſo unintelligible.

He cannot find, that the account of the fits of tranſmiſſion and reflection given in our Remarks previous to this accuſation hath any thing to do with the caſe in hand (*b*); "for adds he, "ſurely no argument can be drawn "from the conſideration of white and compound light, iſſuing from innumerable lucid "points, entering the firſt ſurface of a tranſparent body; and reflected from the ſecond "ſurface, againſt our explication of a caſe, in "which the light is ſimple and uncompounded, "iſſues from one lucid point only, and is reflected back from the firſt ſurface, without "coming to the ſecond."

This aſſertion that in the caſe, he has conſidered (meaning art. 209.) the light is ſimple and uncompounded, is abſolutely falſe: for the light is there ſuppoſed to be the compound light of common viſible objects. As to the other part of his reaſon why our concluſions affect not him, viz. "that we conſider the light iſſuing from "innumerable lucid points entering the firſt "ſurface of a tranſparent body, and reflected "from the ſecond ſurface, whereas he explains "a caſe, in which the light iſſues from one "lucid

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(*a*) Rep. p. 43, at the bottom (*b*) Rep. p. 39. l. 16. and following.

"lucid point only, and is reflected back from "the firſt ſurface without coming to the ſecond," if it means any thing, it muſt mean, that the various lengths of the fits of tranſmiſſion in rays of different ſpecies (the principle, from whence we have deduced the abſurdity of his ſuppoſition) do not take place in the rays emitted from a lucid point, but only after they have been tranſmitted through the firſt ſurface of ſome tranſparent body. If this be really our author's preſent opinion, he will, I hope, abandon it, when he has well conſidered the 13th propoſition of the ſecond book of Sir Iſaac Newton's Optics, where it is evidently ſuppoſed, that the rays of light at their firſt emiſſion from luminous bodies are put into alternate fits of reflection and tranſmiſſion of the very ſame kind with thoſe, they are put into upon paſſing through refracting ſurfaces. Beſides, he forgets, that the light, which enters the cornea, comes to it through another tranſparent ſubſtance, the air.

BUT he proceeds, and, as in our Remarks (*a*) we had aſſerted, that in all common appearances the light reflected from every point of either ſurface of tranſparent bodies is not only always white, but alſo the ſame in quantity of light, he particularly inſiſts (*b*), that this paragraph is either falſe, or utterly foreign to the purpoſe; ſince, if the light here ſpoken of be ſuppoſed to come from a ſingle lucid point, then the light will be reflected in a greater quantity, where it falls more obliquely, and on the other hand, if the light be ſuppoſed to come from innumerable points, as is the caſe in all common

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(*a*) Pag. 103. lin. 18.

(*b*) Rep. p. 39. at the bottom.

common appearances, this paragraph has no relation to the caſe, he propoſes, where the light comes from a ſingle lucid point.

But our author, could he be preſumed to underſtand, even what he himſelf has written, muſt confeſs, that he here endeavours to ſcreen himſelf under the groſſeſt fallacies; ſince he has allowed a few pages afterwards (*a*), that the ſmall inequalities in the quantity of reflected light ariſing from the different obliquities of the ſurface, it is reflected from, cannot in the leaſt contribute to the production of his imaginary dark and bright rings. When he aſſerts (*b*) too, that, if the light be ſuppoſed to come from innumerable points, it can have no relation to the caſe, he propoſes, where the light comes from a ſingle lucid point, he here has ſurely forgot, that the ſole purpoſe of his hypotheſis was to account for the multiple appearance of compound viſible objects, in which he conceives dark and bright intervals are by this means actually produced, where he allows the light to come from innumerable points.

However this gentleman, tho' he afterwards acquieſces in the validity of our objection (*c*), yet here reſolves not to underſtand it, and therefore tells us (*d*), that he has no other way to know my meaning than by turning to the recapitulation at the end of my Remarks, where he finds me to have aſſerted, that his deſcription and application of this doctrine do neceſſarily ſuppoſe the lengths of the fits

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(*a*) Rep. pag. 45. l. 25.

(*b*) Ibid. pag. 39. at the bottom.

(*c*) Ibid. pag. 43. lin. 24. and following.

(*d*) Ibid. p. 40. l. 6.

fits to be the ſame in all rays, however different in colour.

AND to this charge he replies (*a*) by poſitively affirming, that neither his deſcription of this doctrine, nor his application of it, nor his deſcription and application put together do neceſſarily ſuppoſe the lengths of theſe fits to be the ſame in all rays however different in colour.

FOR firſt we are told, that "in the deſcrip-
" tion of this doctrine there is no mention made
" of white or compound rays, ſuch as are com-
" poſed of innumerable ſimple rays different
" in colour, that he ſpeaks ſimply of rays coming
" from a lucid point: and tho' a lucid point may
" be ſuppoſed either to emit compound rays, or
" rays homogeneal and uncompounded, yet the
" whole tenour of his deſcription can be ap-
" plied only to ſuch rays as are either purely
" homogeneal, or principally ſo."

NOW here I obſerve, it is confeſſed, that in the deſcription of this doctrine art. 209. the fits are made of the ſame length in all the rays flowing from the lucid point A, but as this pencil of rays is not expreſsly denominated compound light, therefore it is inſinuated, that it was intended to repreſent homogeneous light only, or principally ſuch.

BUT on the contrary, it is manifeſt, that ſince the pencil of rays deſcribed in art. 209. is not expreſly denominated homogeneous light, it could only be intended by the author to repre-

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(*a*) Rep. p. 40. at the bottom.

ſent the common mix'd light flowing from viſible objects; ſince the concluſions contained in that article are ſolely formed with a view of accounting for the multiple appearance of common objects, and are immediately transferred to any pencil of rays falling on the cornea, and afterwards applied to every pencil of rays flowing from any object whatever without any reſtriction as to the kind of light, or colour, with which it was endued. After this how evaſive and puerile muſt his argument appear, by which he would endeavour to prove, that he could in the inſtance before us mean homogeneal light only, viz. (*a*) "that the whole tenor of his "deſcription can be applied only to ſuch rays "as are purely homogeneal or principally ſo. As if the very error, we had cenſured, was not his having applied to the mix'd light of common objects thoſe deductions, which are only true of light the moſt rigorouſly homogeneal, where all the rays are not only of the ſame colour, but accurately of the ſame degree of colour.

For notwithſtanding our author's aſſertion in this and the next paragraph (*b*), that the concluſions contained in art. 209. are true, provided the light there treated of be nearly homogeneal; yet this amounts to no more than a new proof of his ignorance; for the leaſt inequality in the ſpecies of the rays flowing from A will deſtroy the deductions there framed; but this point we ſhall more particularly diſcuſs a little lower.

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(*a*) Rep. pag. 41. l. 8. (*b*) Ibid. pag. 41. l. 13.

HOWEVER this gentleman does in the next lines acknowledge (*a*), that, tho' the caſe, he has particularly conſidered (meaning in art. 209.) relates only to homogeneal light, yet he has afterwards applied it to phænomena, in which the light is compounded, as the light of the ſtars, of the ſky, &c. whereas he confeſſes, it would have been proper to have applied it in the firſt place to ſuch phænomena, in which the light is homogeneal, or at leaſt principally ſo, after which, and not before (adds he) I ſhould have proceeded to the phænomena produced by compound light. And this proceedure he in the next paragraph ſtiles an omiſſion (*b*), and tells us, it was owing to the haſte, in which he drew up his Eſſay.

BUT how trifling an attempt is this to palliate his errors? he would here inſinuate, that all, that could be imputed to him, was his having omitted to conſider ſeparately the phænomena ſeen by homogeneal and by heterogeneal light, but he well knows, that our cenſure is not founded on what he has omitted to do, but on what he has actually done: we do not accuſe him of having proceeded to the conſideration of objects ſeen by compound light, before he had conſidered thoſe ſeen by homogeneal light, but we accuſe him of having ignorantly applied to the compound light of all viſible objects thoſe concluſions, which are only true of light abſolutely homogeneal, that is of ſuch light, with which alone no object either is, or can by any artifice whatever be illuminated.

(*a*) Rep. p. 41. l. 22. (*b*) Ibid. p. 41. at the end.

After the conceſſion contained in the forementioned paragraph (*a*), viz. that tho' the caſe, he had principally conſidered, and explained, related to homogeneal light only (in which the lengths of the fits are in every ray the ſame) yet he afterwards applied this doctrine to phænomena, in which the light is compounded, &c. after this conceſſion, I ſay, how will he defend, that bold aſſertion in the preceding page (*b*), " that neither his deſcrip-" tion of this doctrine, nor his application of " it, nor his deſcription and application put " together did neceſſarily ſuppoſe the lengths " of theſe fits to be the ſame in all rays how-" ever different in colour?

Dr. Jurin having thus endeavoured to ſhelter his error under the gentle title of an omiſſion, he next tells us (*c*), that he will now ſupply it by laying down ſome phænomena of homogeneal light analogous to thoſe of compound light, which are recited in his Eſſay; and having deſcribed the double and triple appearance of threads of yellow and red ſilk, &c. ſeen by indiſtinct viſion he adds (*d*), " In theſe and many " other experiments of a like nature, where " the light is homogeneous or nearly ſo, the " explication given in the Eſſay by means of " the viciſſitude of the fits of eaſy tranſmiſſion " and reflection will plainly account for all " the appearances."

But the contrary to this is inconteſtably true. Had I not been long accuſtomed to a ſimilar

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(*a*) Rep. p. 41. l. 22.
(*b*) Ib. p. 40. at the end.
(*c*) Ib. p. 42. l. 1.
(*d*) Ib. p. 43. l. 5.

ſimilar behaviour in Philalethes, I ſhould indeed be aſtoniſhed at the unhappy conduct of this gentleman, who, when by his original compoſitions he has convinced the world, he does not underſtand his ſubject, never fails, upon his errors being pointed out to him, to make ſuch a defence, as proves, that after the moſt elaborate attention he is not capable of underſtanding it.

BUT to be more particular, let us firſt preſume, that the light of the yellow or red ſilk is altogether homogeneous, then the concluſions in art. 209. will indeed be valid; but computing from thence what would be the breadth of the rings thus formed on the retina, we have found in our Remarks page 108, that ſuppoſing the ſituation of the rays to be the moſt favourable for the enlarging of theſe rings, conceiving the rings formed at the cornea to be propagated undivided from the cornea, and to be received on the retina placed much more out of the limits of diſtinct viſion, than what our author ſuppoſes; yet with all theſe advantageous conceſſions they would be ſo extreamly ſmall as to be utterly imperceptible.

BUT this pretended homogeneity of the light is a mere fiction; for in reality the light of all bodies, as it depends on the magnitude of their parts, is known to be extreamly compounded, when yet a very ſmall degree of mixture will entirely deſtroy this author's reaſoning; for let us ſuppoſe the light of the yellow ſilk not to be compounded of all the ſpecies of yellow light (tho' it is well known to be made up of various colours) but to conſiſt only of the 50th part of the contiguous ſpecies of yellow, and tho' this light

light is almoſt uniform, and will be allowed by Dr. Jurin, I preſume, to be prodigiouſly more ſimple than the colours of any natural body, or than what can be acquired by any artificial ſeparation of colours whatever; yet, if a pencil of rays formed of this light ſo nearly homogeneous be ſuppoſed to iſſue from a point removed more than one hundredth part of an inch from the cornea, no rings alternately dark and bright would be hereby formed, nor would any ſenſible inequality ariſe in the light reflected at different parts of the cornea; for beyond even this ſmall diſtance from the lucid point the rays, that are in fits of reflection, will be every where equally blended with others, that are in fits of tranſmiſſion.

But our author not knowing that from the heterogenous compoſition even of the ſmalleſt portions of the rays comprehended under the ſame denomination of colour, there would be produced an uniformity in the quantity of light reflected at all diſtances, within which even the moſt indiſtinct degree of viſion can be ſuppoſed to be experienced, he preſumes the appearances of the yellow and red ſilk to be amply accounted for; and after having inſinuated (*a*), that it was the ſolution of theſe ſimple caſes, where the light is, as he ſtiles it, homogeneous or nearly ſo, that he had particularly explained in his Eſſay, tho' but two pages before he has confeſſed the contrary (*b*), he next proceeds to conſider the more compound caſe, and to examine the exceptions, I had made to the account, he had given of this matter in his Eſſay.

And

(*a*) Rep. pag. 43. l. 18. (*b*) Ibid. p. 41. l. 26.

AND notwithſtanding his former complaints againſt the myſterious manner, in which I had framed, what he conſidered as my ſecond objection, he has here (*a*) produced the ſame reaſons, we had urged in that ſuppoſed enigmatic accuſation, and by the force of theſe reaſons he is at laſt driven to confeſs (*c*), that, when a ray of compound or white light falls on the cornea, no dark rings can poſſibly be formed by an unequal reflection at different parts of the cornea.

HERE then the whole point in diſpute is at once given up; for every example and every application of this doctrine in the Eſſay ſuppoſes the light to be compound, nay the very mention of colour, or of the homogeneous or heterogenous compoſition of light does not once occur through all that diſquiſition; and yet the whole ſpeculation, that is there purſued is the adapting theſe rings, which are here confeſſed to be imaginary, to the ſolution of many appearances occurring in objects ſeen indiſtinctly by common light.

BUT, tho' our author has been thus forced to own the fallacy of thoſe reaſonings on this head, with which ſo conſiderable a part of his Eſſay abounds, he will nevertheleſs defend the reality of theſe rings on other principles, and to this purpoſe a new ſcheme is here advanced (*c*), in which they are not ſuppoſed to ariſe ſolely from an inequality of reflection at the cornea, but from the ſeparation of the colours by their diffe-

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(*a*) Rep. p. 43. at the bottom and following.

(*b*) Ibid. p. 45. l. 16.

(*c*) Ibid. p. 46. l. 3. and following.

different degrees of refraction at their entering the different coats of the eye. For we are told, that (*a*), if the compound or white ray of light C D (ſee his figure page 47) falling on the cornea at D has its red light refracted to the point G on the retina, and the compound ray CE has in like manner its red light refracted to g, and its violet light to G; then the point G will be illuminated by ſome of the ſpecies of every ray, that enters the eye between E and D. Now from the different incidences of theſe different rays the extents of the fits in the tranſmitted rays will be varied, and the intervals being alſo different from the points of incidence between E and D to the point G, on which it is ſuppoſed ſome of the ſpecies of each of thoſe incident rays do fall, in conſequence of theſe varieties in the different ſpecies of rays, with which the point G is illuminated, it is by our author ſuppoſed, that in ſome of thoſe ſpecies illuminating G (the citrine yellow for inſtance) the greateſt part of the ray may in one ſituation of that point be in a fit of reflection, and in another in a fit of tranſmiſſion, and that therefore rings alternately dark and bright may by this means be formed on the retina. All this is moſt tediouſly deſcanted on from the 46th to the 54th page of the Reply; and could we, after what we have already ſeen, be at all ſurpriſed at the incorrigible miſtakes of this gentleman, we ſhould doubtleſs wonder, how one, who but juſt before had ſo ſignally experienced his proneneſs to error, ſhould yet without increaſing either his knowledge or his diffidence attempt a ſecond time the inveſtigation of a ſubject, that had once ſo unhappily miſled him.

(*a*) Rep. pag. 49. l. 18. and following.

AFTER what has already been ſhewn in the examination of this Reply, it were paying too little deference to the judgment of the reader particularly to expoſe the numerous fallacies, and ignorant ſuppoſitions, with which this new modelled ſcheme abounds in almoſt every paragraph. I ſhall therefore content myſelf with animadverting on three capital errors, either of which being confuted, the whole ſcheme falls to the ground. And firſt I muſt obſerve, that one principal ſource of his miſtakes is his not knowing in how very ſmall an interval from the radiating point, any rays endued with the leaſt degree of heterogeneity will have thoſe in fits of tranſmiſſion, and thoſe in fits of reflection equally blended together; for would he, or could he properly compute, he would find, that a ray of light containing but the hundredth part of the contiguous ſpecies of the yellow light would not afford any inequality in its reflections at the diſtance of more than one fiftieth part of an inch from the radiating point. How weak then is his conception of a pencil conſiſting of a whole colour, or at leaſt of the greateſt part of it, being to be found altogether in fits of tranſmiſſion or altogether in fits of reflection after paſſing through much greater intervals.

AGAIN Dr. Jurin agrees, that the appearance here endeavoured to be accounted for only takes place in indiſtinct viſion, and undertakes to ſhew (*a*), that the dark and bright rings, to which he ſuppoſes it to be owing, "will be "formed upon the retina, when the luminous point, we look at, is out of the limits "of

(*a*) Rep. p. 46. l. 19.

“ of diſtinct viſion;” and yet there is no circumſtance in his ſolution, but what equally occurs, when the viſion is abſolutely diſtinct. Nay the magnitude of the interval G g (*a*) (on which it ſhould ſeem, the breadth of theſe ſuppoſititious rings ought to depend) may be much greater in diſtinct viſion than in indiſtinct viſion.

BUT laſtly is it not wonderful, that this gentleman ſhould deſcant on the doctrine of light and colours ſo amply, as he has done in his Reply, and yet ſhould in the laſt part of it ſtill endeavour to account for an appearance, which is no ways tinged with various colours, on principles, which, if they were true, would diverſify that appearance with as many colours as the rainbow? For, if rings were formed on the retina, by reaſon that ſome ſpecies of colours being in fits of reflection were reflected back from the ſurfaces of the cornea and cryſtalline, as he here contends, then thoſe rings, which were thus deprived of part of their colours, would doubtleſs appear coloured by the prevalency of their remaining ſpecies. But as it is not pretended, that the objects, to which this doctrine is to be applied, are thus coloured, this conſideration alone proves the ſpeculation purſued by Dr. Jurin to be altogether chimerical. And this proceedure is the more inexcuſable in him, as I had expreſly informed him in my Remarks (*b*), that, if this phænomenon did really depend on the fits of tranſmiſſion and reflection, as he ſuppoſed, then inſtead of conſiſting of dark and lucid parts only, as it in fact does, it ought of neceſſity to appear coloured; to which he has made no reply.

To

(*a*) Reply fig. pag. 47.

(*b*) Pag. 107. l. 27.

To ſpare this gentleman the labour of framing a new hypotheſis, and bewildring himſelf farther in this ſubject I will inform him of ſome particulars relating to the multiple appearance of objects ſeen indiſtinctly, that will not only evince, that the fits of tranſmiſſion and reflection have nothing to do in this affair, but will alſo prove, that the cauſe, I had hinted at in my Remarks (*a*), is undoubtedly the genuine one.

It is true, that a narrow line of light, ſuch as the interval between the edges of a parallel ruler brought very near together, will appear bordered with other lines of light, when held either too near or too remote for diſtinct viſion. If Dr. Jurin's aſſignment of the cauſe of this appearance can at all take place, the ſtars ought not to appear radiated, but invironed with concentric circles of light, as the forementioned line of light is bordered with parallel lines. A very ſmall pin-hole in a piece of paper being held up againſt the light firſt at a diſtance from the eye, wherein it may be ſeen diſtinctly; and then while the eye is directed to it, drawn gradually nearer, this hole, as ſoon as it ceaſes to be ſeen diſtinctly, will alſo appear radiated like a ſtar, not encompaſſed with luminous circles. If the appearances in this experiment are duly attended to, they will account for the lines of light in the other obſervation: for theſe rays will moſt of them, if not all, have a lucid ſpeck in them; ſo that if a ſlit were cut in the paper through the pin-hole of the breadth of the hole, each of theſe ſpecks will become a line of light parallel to the ſlit. But circles

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(*a*) Rem. p. 94. l. 19.

circles of light not appearing about the pin-hole in this experiment, ſhews plainly, that no ſuch luminous circles are formed in the bottom of the eye by rays iſſuing from a luminous point either beyond or within the limits of diſtinct viſion.

THUS have I gone through each particular of Dr. Jurin's Reply; the contemptible impertinence in the three laſt pages excepted. And tho' my cenſures of that gentleman's Eſſay upon diſtinct and indiſtinct viſion publiſhed in my Remarks are ſufficiently juſtified even by the author himſelf, who has given up almoſt every point excepted to; yet it is but juſtice to confeſs, that his preſent performance has made me leſs confident in an opinion, I once entertained relating to this affair. For on my firſt peruſing his Eſſay, the numerous and obvious miſtakes, that occurred in it, induced me to believe, that the author had never read the theory of light and colours in Sir Iſaac Newton's own account of it; but had taken his opinions from hearſay deſcriptions only. However, as it cannot be doubted, but he has now examined Sir Iſaac Newton's Optics with proper aſſiduity during the time of compoſing this defence; the new framed errors, with which this defence abounds, will for the future render it impoſſible to gueſs, what books this gentleman has not read, from the ſubjects he appears not to underſtand.

FINIS.